SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

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MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

ALPHEUS SPRING PACKARD.

ALPHEUS SPRING PACKARD, for twenty-six years professor of zoology and geology in Brown University, died at Providence, February 14, 1905. He was born at Brunswick, Maine, February 19, 1831. His father, for whom he was named, was for over sixty years connected with the Bowdoin College faculty, and his grandfather, the Rev. Dr. Appleton, was one of the early presidents of the college.

At the age of eighteen he entered Bowdoin and there came under the influence and instruction of the late Paul Ansel Chadbourne, who fostered and encouraged his natural inclination towards zoological work. Dr. Chadbourne at this time was also connected with Williams College and it was through him that Packard became a member of the Williams College expedition of 1860 to Greenland and Labrador, with which he went only as far as Labrador, where he spent two months collecting, getting back to college in time for the studies of senior year. In the senior vacation he, with several other Bowdoin students, went on a dredging trip to the Bay of Fundy.

Immediately after graduation in 1861 he accepted the position of entomologist to the newly established scientific survey of Maine and in this capacity he traveled through a large part of the northern wilderness of the state. On this expedition he made the first discovery of Silurian fossils in the northern part of Maine and obtained material for several articles which were published in the first two reports of the survey.

He had now decided on his life work and after the season in the field, went to Cambridge to study with Agassiz. Here he devoted himself largely to the study of insects for the three years that he retained his connection with the Museum of Comparative Zoology, but in his spare time he read medicine and each winter he attended the lectures in the Maine Medical School connected with Bowdoin, from which he was graduated with the degree of doctor of medicine in 1864.

In the summer of that year he made a second trip to Labrador, where, with his enlarged experience, he was able to add greatly to the knowledge acquired on his former trip. As a result, besides several smaller papers, he published a large memoir on the geology and zoology of that region. Later this material was worked over and formed the basis of his book on 'The Labrador Coast.'

On his return from this second trip to Labrador he enlisted for three years as assistant surgeon and accompanied the first regiment of Maine Veteran Volunteers to Virginia, where he served until the end of the war. These ten months included the whole of his medical practise. After being mustered out he acted for a time as librarian and custodian of the Boston Society of Natural History, remaining there until 1866 when with several of his former fellow students—Hyatt, Morse, Putnam and Cooke—he accepted a position in the museum of the Essex Institute at Salem, at that time one of the most active scientific societies in the country.

Then came the founding of the Peabody Academy of Science in Salem. To it the Essex Institute transferred its collections and the scientific corps went with them, Packard being appointed curator of invertebrates and in 1876 director of the academy. Here he remained until 1878, when he accepted the position at Brown which he held for the rest of his life. While at Salem he held various other positions. He was for three years state entomologist of Massachusetts, lecturer for several years in entomology at the Amherst and Orono Agricultural Colleges, and for two or three years upon zoology and comparative anatomy at Bowdoin College. He also worked for two summers on the *Bache* and *Blue Light*, dredging for the U. S. Fish Commission in the Gulf of Maine. He was connected for a time with the Kentucky Geological Survey, when he made a zoological exploration of Mammoth Cave and laid the foundation of his later work on cave life. From 1875 to 1877 he was one of the zoologists of the U. S. Geological Survey under Hayden.

In 1873 Agassiz inaugurated the Anderson School of Natural History on the island of Penekese, the first summer school of biology in America. Here for two years Packard gave the instruction in insects and crustacea, and when with Agassiz's death the school was given up, Packard started a similar but smaller summer laboratory at Salem under the auspices of the Peabody Academy of Science which he conducted until his removal to Providence. This work was later taken up by the late Professor Hyatt at Annisquam, Mass., and continued until the establishment of the Marine Biological Laboratory at Woods Hole. In 1876 he was appointed by the President a member of the U. S. Entomological Commission which was to devise ways and means of checking the ravages of the Rocky Mountain locusts in the trans-Mississippi country. Later the scope of the commission, which lasted for five years, was enlarged so that it might deal with other insect pests. On this service he made two trips to the west, one taking him to the Pacific coast.

Besides these trips he spent the winter of 1869–70 in Florida, stopping on his re-

turn at Beaufort, N. C., from which place he brought back large collections of invertebrates. The next winter he spent at Charleston, S. C., where he studied the development of numerous marine invertebrates and especially of the crustacea, and where he collected the tertiary molluscs made known by F. S. Holmes. In 1872 he visited Europe, studying the collections of insects in the large museums and paying especial attention to Walker's types of lepidoptera in the British Museum. In 1885 he visited Mexico and in 1898 again spent a year in Europe and northern Africa.

Dr. Packard was a most indefatigable worker, the list of papers which came from his pen being numbered by hundreds. Only a few of these can be mentioned here. His first article was upon the army worm and was published by the Maine Scientific Survey. The years at Cambridge were chiefly spent in study, but some of the notes then made were incorporated in numerous later works, although large numbers of observations made in these early years remained unpublished at his death. His first large work was the monograph of the geometrid moths published by Hayden's Survey, and scarcely less imposing was his account of the Bombycidæ issued by the National Academy. His embryological work, which included studies on the development of the lower insects, appeared in the 'Memoirs' of the Peabody Academy of Science and in minor papers elsewhere, while his memoir on the development of the horseshoe crab remained for years the chief source of our knowledge of that interesting animal. This work was all done before the days of sections and was based entirely upon surface views and optical sections, a fact for which allowance should be made when his mistakes are recalled. His papers on the geology and natural history of Labrador and on the cave animals have already been alluded to. Possibly his best article was the 'Monograph of the Phyllopod Crustacea' published in the last report of Hayden's Survey.

Packard was possibly best known for his text-books. The earliest of these was his 'Guide to the Study of Insects,' which for years served as the *vade mecum* of hundreds of budding entomologists. Then came his 'Life Histories of Animals,' which was the first attempt since the day of Agassiz's Lowell Institute lectures to summarize the facts of embryology, a work which was early superseded by Balfour's admirable treatise. Then came his 'Zoology,' the first attempt to give American students a truly scientific text-book in which morphology and classification were given equal prominence. This was followed by several smaller and more elementary works for lower schools, some of which have had a large sale. Later came a second work on entomology, in which the morphological side of the subject was strongly emphasized.

Packard, along with his friends Cope and Hyatt, must be regarded as one of the founders and chief supporters of the so-called Neo-Lamarckian school of evolution, and his writings in advocacy of these views are numerous. His studies in this direction led him to study deeply the writings of Lamarck and later to bring together all the known facts in the life of this early apostle of evolution. In fact his second trip to Europe was largely for the purpose of ascertaining everything possible concerning the man.

In speaking of Dr. Packard one should not forget the services he rendered to science as one of the founders and for twenty years as editor of the *American Naturalist*. Almost as soon as he reached Salem the magazine was launched and while one by one the other editors dropped out Packard remained in charge. In these days of numerous natural history magazines one can

hardly realize the boon the establishment of this journal was to the naturalists of the country, and few know its financial vicissitudes and the sacrifices of its editor during its early days.

Personally, Dr. Packard was one of the most companionable of men. He was always ready to aid and assist the young in their natural history studies to the extent of his powers. He was critical of the language in which they clothed their facts and the pages of the *Naturalist* have profited by his revision. He rarely indulged in controversy, and although he could say sharp and cutting things, one may look in vain in his published works for any traces of polemics.

Dr. Packard was married in 1867 to Elizabeth Derby, the daughter of the late Samuel B. Walcott, of Salem, who, with four children, one a rising naval architect, survives him.

J. S. KINGSLEY.

TUFTS COLLEGE, MASS.

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*ALPHEUS SPRING PACKARD.**

I have not known Professor Packard as long, nor as intimately, as many of my colleagues; and where they have spoken I should remain silent. Neither am I qualified to discuss his more immediate scientific work. I can, however, in response to the President's suggestions, speak of him in the light in which one scientific man sees another, older and wiser than himself; but I do so with diffidence. I have, therefore, written down with some care the things which I would not otherwise venture to express.

It seems an ungracious confession to make, but it is nevertheless true, that it was through Professor Packard that many of us in Washington, twenty or thirty years ago, became aware of the existence of scientific activity at Brown University. For age had wearied the enthusiasm of Alexis Caswell twenty years earlier. Yet it was not by his presence that Packard represented her; at least in the years in which I knew him, he was not a frequent attendant at scientific meetings remote from Providence. It was his untiring and remarkably pervasive industry that confronted us. The president of the National Academy, the director of the Geological Survey and others in authority all felt the force of it; and at one time there were dismal mutterings in the high places of legislation asking why the public printer's time should be spent in bringing out the elaborate researches of one who stood remote from public office. How did this come about? Certainly a man of Professor Packard's singular modesty, of his almost morbid habit of self-depreciation, was the last to find his way through the mazes of a government lobby. His transparent sincerity would have been infinitely removed from all this. And yet there was no mystery about it. It was a mere force from within breaking its way. The power of Professor Packard's intellect bearing on subjects of natural history, the scope and accuracy of his learning and the purity of his scientific ideals were his only resources; and wherever institutions needed the fruits of ripe scholarship to dignify their own scientific activities, these were the first to feel the influence of Professor Packard's productive zeal, as they were compelled to guide its progress. And so our unobtrusive colleague taxed the people of the whole United States to publish his magnificent memoirs—because he was genuine.

The same facts appear in a different way, in the further story of Professor Packard's life. I am the last man to speak lightly of the young vigor and the promise of our American institutions, or of our

* Address given at the memorial exercises at Brown University. Printed in SCIENCE at the request of the editor.

learned societies. But it is nevertheless true that in comparison with the famous academies of the old world we are as yet mere children. In a history of the *essentials* in the progress of science, there is but rare need of the mention of American accomplishments. We have much of the practise, and we show a degree of independence in our imitations; but we lack the philosophic depth, the intuitions and the profound originality. It is to the lawgiver of science that the true academy is born, and it is by her lawgivers again that it must be nurtured. To men of exquisite genius no climate within the whole range of our immense country has yet been congenial.

We are apt to smile at the Englishman for the letters which decorate his name. We laugh at the German for his titles and at the Frenchman for his ribbons and his uniform. We smile because to us such insignia mean nothing; and it is to our shame. We forget that these symbols voice a sentiment of almost religious purity. We have not yet learned to constitute nor even to revere a tribunal so august as to be incompatible with pettiness. We never ask why the F.R.S. is inseparable from the names of Lord Kelvin, of Lord Lister, even in their age and amid the splendors of their glory. To make the French Academy, even on its scientific side, required the brains of Cuvier, of Lamarck, of St. Hilaire, of Buffon, of the brothers Jussieu, of Pasteur; it required Laplace, Lavoisier and Lagrange, Carnot and Cauchy, Fresnel and Fourier, Ampère and Arago, Poisson and Poinsot, to mention only a few; and the dictum of the academy arbitrates with the authority of these tremendous names.

Precisely to such bodies of inexorable critics did the intrinsic strength of the work of Professor Packard ultimately appeal. And it was from the judgment of his confrères, from the men who had themselves traversed the same intellectual territory and knew it, that he reaped his supreme honors. From these alone could the reward have come; for below the decisions of his peers, there was no other guide but conscience.

Few of us realize how difficult it is, what persistent convictions, what sturdy vigilance is required to enter seriously into competition with the whole world, as Packard did; indeed one might say to enter handicapped, against a world richer in its traditions, more refined in its higher intellectual atmosphere, more bountiful in its opportunities, than our young country. It takes courage to press forward alone, self-reliant, misunderstood, at peace only with one's own convictions. Did we think of this in Packard's case? Did we look at his Linnean and other honors in this light? Did even our corporation feel that the *cause* of which it is the supreme guardian, had in Packard been awarded with the most cherished tokens of the world's approval?

Packard was not lacking in his reverence for art, for literature, for music; but his soul cried out for science. He felt instinctively that the handiwork of man, however sublime, can not be more than human; and that a finite brain has fashioned all its cultures. Nature is the offspring of omniscience. He realized what the world was so slow to realize, what only within the last few hundred years has come like a tumultuous awakening, that the universe was wrought in the workshops of God, and that she alone is ultimately divine. He felt too that her true poetry is not written in rhetoric but in mathematics and in the stern logic of science. For all our natural philosophies are but an attempt at a picture. We find no adequate symbols in our efforts to restate her methods; our analogies, our metaphors, are gross; we have to shift, to approximate, to neglect. But nature neg-

lects nothing! To her the infinitely large and the infinitely small on the boundaries of which we live are alike finite among her infinities. Touch her at any point and your contact is with the eternal.

To contemplate the prolific labors of Professor Packard is to stand face to face with the attributes of genius. I do not wish to make an over-statement. True, there is an order of genius among the geniuses, but there is none in whose heart the sacred fire does not burn. There can be no holier joy than the joy of creative work, and yet it is a joy akin to terror. What is it which possesses a man even in early youth, which impels him despite all obstacles and restraint to strive evermore, intellectually alone, without approval, profitlessly after an unattainable ideal; whose spell grows more potent as his years ripen, as his toil increases, as the world grows caustic in its rebuke; and that leaves him only with death? Do not suppose that the poet or the sculptor or the martyr alone have it. It burns to-day with subdued passion but with all its pristine and unmitigated fierceness in the life of every true student of nature.

What is it that can sustain a man when every new avenue of thought discovered is but the approach to countless avenues beyond; when to finish, be it after years of labor, is only to be ready to begin; what encourages him when the unknown looms with greater vastness as the known is more profoundly mastered; when the very pinnacle of attainment is the sublime consciousness of ignorance, and when to be most renowned is to be most devoutly humble? It is the inspiration which illumined the life of our friend, our colleague, our teacher. Long may his ideals guide us at Brown!

CARL BARUS.

BROWN UNIVERSITY.

THE ASTRONOMICAL AND ASTROPHYSICAL SOCIETY OF AMERICA.

THE sixth meeting of the society was held December 27–30, 1904, at Philadelphia, Pa., during convocation week, in affiliation with the American Association for the Advancement of Science.

Three sessions of the society for the reading and discussion of papers and the transaction of business were held in room 106, College Hall, University of Pennsylvania, on Wednesday, Thursday and Friday afternoons. The number of members present at some time during the meeting was thirty-six and the average attendance was about fifty.

A pleasant social feature connected with the meeting was an informal dinner at the Hotel Walton, Thursday evening, at which twenty-six members and friends were present. Through the courtesy of Director Doolittle, a number of the members had the pleasure of examining the equipment of the Flower Observatory of the University of Pennsylvania at Upper Darby, and by the courtesy of Professor Snyder the extensive astronomical equipment of the Philadelphia Observatory was inspected by a considerable party.

During the meeting five new members were elected. The selection of a time and place for the next meeting was left open for future action by the council.

The officers elected were:

For 1905:
President—Simon Newcomb.
First Vice-President—George E. Hale.
Second Vice-President—W. W. Campbell.
Treasurer—C. L. Doolittle.

For 1905–6:
Councilors—W. S. Eichelberger, Ormond Stone.

On account of the contemplated absence from the country of G. C. Comstock for the greater part of the year 1905, W. S. Eichelberger was elected by the council as acting secretary.

W. S. Eichelberger and C. L. Doolittle represented the society in the council of the American Association for the Advancement of Science.

By request of a committee of the National Academy of Sciences appointed to secure international cooperation in solar research, a committee from this society was appointed by the president during the preceding summer to cooperate with the committee of the National Academy. The council has made this a standing committee of the society.

PAPERS PRESENTED.

C. L. DOOLITTLE: 'The Constant of Aberration.'

JOHN F. HAYFORD: 'A Test of The Transit Micrometer.'

ERIC DOOLITTLE: 'Remeasurement of the Hough Double Stars.'

D. P. TODD: 'Novel Design for Rotating Dome Track.'

EDWARD S. KING: 'A Study of the Driving Worms of Photographic Telescopes.'

C. L. DOOLITTLE: 'The Reflex Zenith Tube.'

ANNIE J. CANNON: 'Variations of the Bright Hydrogen Lines in Stellar Spectra.'

HENRIETTA S. LEAVITT: 'Variable Stars in Large Nebulous Regions.'

PERCIVAL LOWELL: 'Planetary Spectrograms, the Work of V. M. Slipher and C. O. Lampland.'

PERCIVAL LOWELL: 'The Canals of Mars: An Investigation of Their Objectivity.'

FRANK H. BIGELOW: 'Note on Three Solar Periods.'

JOHN A. PARKHURST: 'The Coordination of Visual and Photographic Star Magnitudes.'

HEBER D. CURTIS: 'The Quadruple System of Alpha Geminorum.'

HAROLD JACOBY: 'Use of the Method of Least Squares to decide between Conflicting Hypotheses.'

HAROLD JACOBY: 'Tables for the Reduction of Astronomical Photographs.'

EDWARD C. PICKERING: 'Recent Researches of the Henry Draper Memorial.'

ORMOND STONE: 'Calibration of a Photographic Photometer Wedge.'

J. G. HAGEN: 'Note on Two Variable Star Catalogues.

'Useful Work for a Small Equatorial.'. A proposed discussion. To be opened by Edward C. Pickering.

ABSTRACTS OF PAPERS.

The Constant of Aberration: C. L. DOOLITTLE.

The systematic observation for variation of latitude was begun by the author December 1, 1889. This work has been kept up with some interruptions since that time. In 1896 was begun at the Flower Observatory a series which it was proposed to continue on a uniform plan for a period of seven years. This design was carried out with but little departure from the original program. Observation on this series was terminated December, 1906.

Work is now in progress on a more comprehensive plan, two instruments being employed, viz., the $5\frac{1}{2}$-inch zenith telescope and the 8-inch Wharton reflex zenith tube.

The close of the former series seems to mark the proper time for bringing together the values of aberration constant which have been obtained, and for combining them to form a mean which may be considered final so far as may be shown by these observations.

The different values found are as follows:

SAYRE OBSERVATORY, SOUTH BETHLEHEM.

Date.	Aberration.	No. Pairs.	Wt.
1889 Dec. 1—1890 Dec. 13	20″.448±014	1479	0
1892 Oct. 10—1893 Dec. 27	20 .551±009	2900	1
1894 Jan. 19—1895 May 16	20 .537±014	1989	1

FLOWER OBSERVATORY, PHILADELPHIA.

Date.	Aberration.	No. Pairs.	Wt.
1896 Oct. 19—1898 Aug. 16	20″.580±008	2009	1
1898 Oct. 8—1899 Nov. 27	20 .540±010	1503	2
1900 May 5—1901 Aug. 30	20 .561±008	1994	2
1901 Oct. 3—1902 Dec. 1	20 .513±009	1935	2
1903 Jan. 22—1903 Dec. 7	20 .524±009	1554	2

The reasons which have led to assigning the wt. 0 to the first determination will be found fully set forth in connection with the published results of this series.* For various reasons which are fully explained elsewhere the first series at the Flower Observatory is not thought to be as reliable

* *Transactions of the American Philosophical Society*, Vol. XX., p. 318.

as the following ones. It has accordingly been assigned the wt. 1, the four remaining values being given the wt. 2. Combining according to these weights, we find for the mean,

$$20''.540 \pm .0055$$

I wish this to be regarded as the definitive value of this constant as derived from the zenith telescope observations extending from December, 1889, to December, 1903.

The Test of a Transit Micrometer: John F. Hayford.

When, in connection with an astronomical transit as used for time observations, a transit micrometer and chronograph are substituted for a system of fixed lines in the diaphragm, a telegraphic key and a chronograph, the observer is relieved of the necessity of operating the key at, or as soon as possible after, each of the several instants of transit of the star across the fixed lines. Instead, he is required simply to keep the star image bisected continuously by the movable micrometer line during its progress across the field of view. In the new process of thought the element of time enters only in an indirect manner. Hence, with a transit micrometer the personal equation becomes so nearly zero, and its variation so nearly zero, that it is difficult to prove that they are not both absolutely zero. The personal equation is one of the most serious sources of error in all time determinations and determinations of right ascension. The destiny of the transit micrometer is to produce a decided increase in accuracy in this class of observations without increase of effort or cost.

The observation of star transits by means of a movable transit line was first suggested in 1865 by Director Carl Braun of the Kalocsa Observatory. He believed that it was necessary to have the movable line driven by clockwork. He failed to construct a satisfactory apparatus.

Repsold, the well-known instrument-maker, was the first to suggest in print, in 1889, that no clockwork is required. He constructed a hand-driven transit micrometer with which excellent results were secured.

The Prussian Geodetic Institute put the Repsold hand-driven transit micrometer into use on portable instruments in making telegraphic longitude determinations in 1891, and has continued its use to the present time. In all, it has been used in ten longitude determinations.

Utilizing the published past experience with transit micrometers Mr. E. G. Fischer, chief of the Instrument Division, Coast and Geodetic Survey, designed and constructed in the winter of 1903–4 the transit micrometer which is before you, and which is adapted for use on the transits ordinarily used in longitude determinations.

It is a hand-driven transit micrometer. It is so well designed and constructed that in the extensive tests, to which I will refer in a moment, it never required the slightest change in adjustment, not even of the pressure of the contact spring, and not a single record was ever lost on account of any failure of the transit micrometer to operate properly.

A peculiar and important feature of this transit micrometer is an automatic switch which operates, without the slightest attention from the observer, in such a manner that a record is made on the chronograph for the middle four turns of the field, and for those turns only. This positively identifies those four turns, keeps the chronograph sheet clear, and enables the observer to practise following the star during the earlier part of its transit without affecting the chronograph sheet in any way.

In March, April and May, 1904, this instrument was tested by 75 time sets on 18

nights at the Coast and Geodetic Survey office. Sixteen observers took part in this test. The observers were purposely selected so as to include some with little or no experience in any kind of observation, some with long experience in astronomic observations and in handling various instruments of precision, and some of various grades between these two extremes. Two observers worked at the same time, observing alternate stars, and thus obtaining a determination of their relative personal equation. One of the sixteen observers was in the test continuously, became thoroughly accustomed to the instrument and method of observation, and served as an intermediary through which all the other observers could be compared with each other.

The tests show that for a practised observer with such a transit micrometer, the total error for a star, including errors which are constant for all the records as well as the accidental errors of bisection, is nearly the same for stars of all declinations if expressed in angular measurement. This is what should be expected if the errors concerned are of the same nature as if the object pointed upon were stationary instead of moving.

The accidental errors of bisection are nearly the same expressed in angular measure for stars of all declinations up to 59°, and are probably somewhat less for stars of greater declination. This is an indication that the accidental errors of bisection are of the same nature as if the image pointed upon were stationary, the indication being partly contradicted by the smaller errors for stars of declination greater than 58°.

Good observations can be secured at once with the transit micrometer without previous practise. Practise simply reduces the accidental errors by about 25 per cent. I feel that I may speak with assurance on this topic, for each of the sixteen observers was forced to begin observing on the first star that appeared in his field of view, with no previous experience whatever. This point is emphasized for the reason that I had been led to expect that long practise would be necessary before an observer could be sent to the field with a transit micrometer. The accidental error of a single record with the transit micrometer is about the same as that of a single record with a key.

During the first half of the tests the driving heads were geared to make one turn in $2^s.4$, when observing an equatorial star. During the last half of the tests the driving heads were geared to turn one half as fast, namely, one turn in $4^s.8$. This extreme change in speed produced surprisingly little effect on the accuracy of the result. With this instrument the speed of $4^s.8$ per turn, or possibly a slightly slower speed, is believed to be most favorable to accuracy.

The tests show that the relative personal equation between any two observers with the transit micrometer is so small as to be masked by the accidental errors of observation. This is equivalent to saying that it is probably less in every case than $^s.05$, and is, as a rule, much smaller than this. The relative personal equation with a transit micrometer is certainly not more than one tenth as large, upon an average, as with a key. This conclusion as to the relative personal equation applies to inexperienced as well as experienced observers.

The literature of the transit micrometer shows abundant corroboration of these conclusions as to the relative personal equation.

It is difficult to detect constant or systematic errors of any kind in transit micrometer observations. All the errors seem to belong to the accidental class.

This is far from being true of key observations.

The transit micrometer is about to be put into use in the regular longitude work of the Coast and Geodetic Survey.

I predict, basing my prediction upon the general experience with transit micrometers as well as on these particular tests, that with a transit micrometer three nights of observations without an exchange of observers will give as great accuracy as has been secured in the past from ten nights of observations with a key, including an exchange of observers. This is a prediction of which the truth or falsity can only be proved conclusively by field experience. I rely upon such experience to be gained within the next five years to verify the prediction.

I venture to predict also that the evidence in favor of the transit micrometer will accumulate to such an extent in the next ten years in fixed observatories, as well as with portable instruments, that the astronomer who uses a key in 1914 for accurate time determinations or determinations of right ascension will have difficulty in furnishing adequate explanation of his conduct.

An illustrated description of the Coast and Geodetic Survey transit micrometer, with a full report of the tests referred to above, and a brief résumé of a part of the literature of the transit micrometer, is now being printed as an appendix to the Coast and Geodetic Survey Report for 1904.

Remeasurement of the Hough Double Stars: Eric Doolittle.

The catalogues of new double stars published by Professor Hough comprise 622 pairs, of which 77 are closer than ½″ and 143 closer than 1″; in those pairs in which the distance is greater than 5″ the companion is usually excessively faint; in fact, there are few of the stars which would not be difficult with a telescope of much less than 18 inches aperture.

The measurement of this fine series of doubles seems to have been strangely neglected. On a few of them, which are of the type of close pairs of equal magnitude, as 98, 260 and 296, there are a number of rather discordant measures, but the great majority have received no attention except from the discoverer himself. Thus there are but 87 pairs which have been measured in two different years, and on no less than 358 there is but a single prior measure.

The entire list was, therefore, added to the observing list for the 18-inch refractor of the Flower Observatory. Thus far, 360 pairs have been measured on three or more nights and many of the remaining 262 are partially measured; a single night's measure consists in each case of at least four measures of position angle and four of the double distance.

Change has been found in 16 of the close pairs, and among the wider ones there is in 33 instances indication of proper motion.

It is the intention, when the work is completed, to publish a catalogue of these stars, including about twenty new pairs which Professor Hough has discovered since his last list was issued.

A Study of the Driving-worms of Several Photographic Telescopes: Edward S. King.

In following a star with a photographic telescope we must have for the period of the exposure a clock the hour hand of which will indicate the elapsed time on a scale graduated to seconds or less. We must have the equivalent of being able to determine the time by measuring the position of the hour hand with a micrometer. If any periodic error occurs in the train of the driving mechanism, causing the telescope to be first in advance of, and then

behind, its proper position, the stellar images will be elongated into lines having a length dependent upon the amount of the oscillation. If the telescope follows the star only at one extremity of the oscillation, we shall have a series of images separated by trails, or, if the rate of the telescope is changed more, we shall have a trail with dark knots appearing at regular intervals. The number of the knots determines the frequency of the oscillation, and almost invariably indicates the driving-worm or endless screw as the offending member.

Such a periodic error, as shown by slide 1, is present in nearly all telescopes driven in this manner. This fact is not anything new, but has been recognized for years. The first example that I know of personally occurred in 1888 with the Boyden thirteen-inch telescope. In 1896 the director asked me to determine the periodic error of two of our photographic telescopes. Several series of measures were made of the eight-inch and the eleven-inch Draper telescopes. The method was to view a point of the tail-piece through a fixed microscope fitted with a micrometer. After each release of the detent by means of the signals given by hand, the position of the point was read and recorded. The reduction of these measures shows that the oscillation for the eight-inch Draper telescope was about 1 second, and for the eleven-inch Draper telescope about 0.2 second. These figures correspond to trails of less than 0.01 cm. on the plate. Within a few years Dr. Hartman has studied the periodic error of the Potsdam refractor and provided a very ingenious method of correction. A full account of his work will be found in the *Astronomische Nachrichten,* No. 3,769, page 2.

Nothing further was done here until the present year, when one of the small cameras was provided with a new mounting. The images proved to be lines lying in the direction of the clock's motion, and might, therefore, be affected by a periodic error. I proceeded to investigate the difficulty by a photographic method. The polar axis of the instrument was displaced in azimuth by a large amount. Such a displacement would cause equatorial stars, particularly when near the meridian, to move over the plate in declination. If an oscillation occurred, it would appear in the sinuous character of the trail. Slide 2, which is enlarged ten times from the original plate, shows the result, permitting no doubt as to the nature of the error. The numerous elongated objects are images of stars obtained on the same plate in the ordinary way. It is seen that the elongation of the images corresponds to the amplitude of the oscillation as exhibited by the vertical trail. The number of the oscillations was fifteen per hour, which fixes the responsibility upon the worm. A similar experiment made with a worm which had given satisfactory images is shown in slide 3. A slight irregularity is seen, but does not prove injurious. The same experiment made with the eleven-inch Draper telescope did not show anything definite, due probably to the smallness of the error. It is possible that it could be brought out by attaching an enlarging apparatus to the instrument and this will be tried soon.

The defective worm and several others were also tested visually. A telescope of about four feet focal length and having an eye-piece provided with a crosswire, was lashed to the camera and directed to a scale graduated to millimeters and placed at a distance. I was thus able to record the position of the telescope accurately. After every ten beats given by hand to the driving mechanism, the position was read for a period covering more than a revolution of the worm, which occurred in 240 seconds. The readings at

the beginning and the end of the revolution determined the average rate from which were found the positions which the telescope should have occupied for the intermediate times. A comparison with the observed positions gave the periodic error. It was possible for the worm to be defective either in the thread itself or in the mounting. Rowland makes the remark that the correct mounting of a screw is more difficult than making the screw. It seemed to be so in the present case. If the screw was mounted eccentrically, we might expect a great improvement if it were allowed to engage only lightly with the R. A. wheel, being held in position by a strong spring. Slide 4 exhibits the resulting curves. It is seen that when the worm was adjusted to engage only lightly with the R. A. wheel, the oscillation extended through nearly eight seconds, but that, when brought into contact, the range was hardly two seconds. Thus the error of this particular worm was eccentricity.

A further test was made to determine if any intermediate adjustment of the worm with respect to the R. A. wheel would be advantageous. Slide 5 gives a set of curves for the various settings, beginning with the position in contact and ending with the worm very lightly engaged. Curve 1 is best and curve 3 is worst. That the position in contact is not always best is shown by slide 6, giving results for the worm that proved satisfactory. In this case the action was quite erratic when the worm was in contact.

A further test was made of the eleven-inch Draper telescope to determine the best adjustment of the worm with respect to the R. A. or sector wheel. Slide 1, which has already been seen, shows the appearance of the trails at what may be considered an average adjustment. In slide 8 we have the result when the worm is in contact, and in slide 9 when the position is adjusted to give the least error, found by trial. Probably in all instruments, one may, without any process of reconstruction, find by experiment where the periodic error is much decreased.

The foregoing has been in the nature of an abstract of an investigation in progress rather than a detailed account of work complete. It is possible that a careful study of curves representative of the action of the driving-worm will suggest an improvement in cutting the thread. As the experiments are easily made, I hope that other observers will test their instruments. A comparison of the results obtained with a greater variety of instruments would be of interest and might lead to a better understanding of the entire subject.

The Reflex Zenith Tube: C. L. Doolittle.

In 1851, or thereabout, the instrument having this designation was installed at Greenwich. The maker was Mr. Simons, the designer Mr. J. B. Airy. The immediate object in view was the observation of γ *Draconis,* a star which has been followed at Greenwich with some kind of zenith instrument since the time of Bradley.

The principle is briefly as follows: The telescope is fixed permanently, with its axis vertical as nearly as may be. Below the objective at a distance nearly half that of the focal length is placed a basin of mercury. The rays from a star at the zenith after passing through the objective, are reflected from the mercury surface and brought to a focus immediately in front of this objective. By means of a micrometer, the frame of which is firmly attached to the cell of the objective, with the plane of the reticule passing through the focus, the zenith distance of a star culminating within ten or fifteen minutes of the zenith may be measured. Finally a diagonal reflector brings the ray to the

ocular, which is at right angles to the axis of the objective.

In practise the observation is made by bisecting the star, then quickly reversing by turning the objective with the micrometer attached through 180°, then making a second bisection. One half the difference of the micrometer readings will evidently be the measure of the star's zenith distance. Obviously both bisections can not be made with the star on the meridian. This makes necessary a small correction easily determined.

An instrument involving these principles has recently been installed at the Flower Observatory. So far as I am aware this is the second to be constructed. In detail, it differs in a number of particulars from the Greenwich instrument. The optical parts are by Brashear, and the instrumental parts by Warner and Swasey. The aperture of the objective is eight inches, the focal length one hundred inches. In the Greenwich instrument this cone of light after reflection passes a second time through the glass of the objective. In this case a hole one and one half inches in diameter is bored through the objective; through this hole a short tube passes, attached above to the micrometer box. When not in use this tube is closed by a shutter which presses up against its lower end, thus protecting the reticule from dust and moisture as completely as in the ordinary form of telescope. The construction may be likened to that of an ordinary telescope with the tube cut in two near the ocular, this end being passed through a hole through the middle of the objective.

Another matter of importance is this. It is evident that unless the plane of the reticule passes through one of the principal points of the objective, any change in the inclination of the apparatus will shift the zero point of the micrometer with respect to the vertical. This makes necessary a correction depending on the level readings—the very thing which we wish to avoid. For this purpose Dr. Hastings, who computed the curves of this objective, so designed it as to bring the first principal point in front of the upper surface, 0.155 inch. It was a simple matter to place the plane of the reticule at this same distance. These peculiarities introduced into the problem some technical and mechanical difficulties, all of which were successfully overcome, the optical performance being entirely satisfactory.

A solid cast-iron pillar, weighing several hundred pounds, formed the tube of the telescope. The focal adjustment is made by raising or lowering the mercury surface, this arrangement offering no difficulties.

As is usually the case with a new design, in part experimental, various unforeseen delays have occurred. All previous difficulties have apparently been overcome, and regular observations are now in progress. At present observations are carried on simultaneously with this instrument and the zenith telescope; four groups are employed as heretofore. In this program each group contains eight zenith stars to be observed with the reflex tube, and ten latitude pairs for the zenith telescope, with one wide pair for temperature investigation; the time required for these nineteen observations being on the average approximately two and one half hours. It is hoped that in the course of two or three years these observations may furnish data tending to throw light on a number of obscure problems.

The star γ *Cygni*, magnitude 2.5, culminates within less than one minute of the zenith of the instrument. Although at present it differs in right ascension from the sun by only $1^h\,30^m$, it is an easy object to observe. There will be no difficulty in following it during the greater

part of the year. As an indication of the performance of the instrument the latitudes resulting from a preliminary reduction of the observations made on this star are here given.

1904, Dec.	6,	$\phi = 39° 58' 1.83''$
	7,	1.95
	8,	1.85
	9,	1.86
	13,	1.81
	14,	1.79
	16,	1.84
	18,	1.92
	31,	1.79
1905, Jan.	1,	2.00

Variations of the Bright Hydrogen Lines in Stellar Spectra: ANNIE J. CANNON.

Stars whose spectra are of the Orion type, having also one or more bright hydrogen lines, form a most interesting peculiar class whose position in the scheme of stellar evolution is enigmatical. The Harvard photographs show that the bright hydrogen lines are variables in the following six of these stars, *η Centauri, κ' Apodis, υ Sagittarii, ε Capricorni,* J *Velorum* and 27 *Canis Majoris.* So far as known, no variation in the light of any of these stars has ever been observed, although the changes in their spectra point either to great atmospheric upheavals or to movements of two or more revolving bodies.

The most important changes in the spectrum of *η Centauri* may be summarized as follows: In 1897 all lines were dark and $H\beta$ was nearly as intense as $H\gamma$. In 1898 and 1899 $H\beta$ was very faint and appeared as a dark line superposed on a faint bright band. In 1901 a most striking change had taken place, for $H\beta$ had become a strong bright line, having considerable shift towards the violet when compared with the dark $H\beta$ present in 1897. $H\gamma$ was dark with a bright band towards the violet. Photographs taken in 1902 recorded the reappearance of the dark line on the edge of greater wave-length of bright $H\beta$, and both lines were of moderate intensity. In 1903 the spectrum was similar to that of 1898. The period of these changes is probably several years in length.

The changes in the spectrum of *κ' Apodis* are somewhat similar to those of *η Centauri.* It appears that both these stars are spectroscopic binaries, one component of each being a bright line star. The spectrum of *υ Sagittarii* presents another difficult spectroscopic problem, perhaps on the order of *β Lyræ.* The spectrum of *υ Sagittarii* always appears to be composite. The principal lines seem to be due to two bodies, one having a spectrum like *β Orionis* and the other like *ε Aurigæ.* The spectrum of *β Orionis* was strongly predominant on seven photographs, but frequently the two spectra seemed to be equally intermingled. Perhaps the most curious phenomenon is that on twenty-three photographs, on which the helium lines were very strong, those of hydrogen were unusually weak. $H\beta$ was invisible, appearing neither as a line of emission nor of absorption, while line 4,922 was clearly seen. $H\gamma$ and $H\delta$ were respectively much less intense than the adjacent helium lines at 4,387.8 and 4,120.5. It is possible that a third body, having bright hydrogen lines, might explain these appearances.

Eleven photographs of the spectrum of *ε Capricorni,* taken in 1903, showed $H\beta$ to be a faint but distinct bright line lying on the edge of greater wave-length of an equally faint dark line. On earlier photographs, $H\beta$ was dark and of varying intensity. Some faint lines, including several due to iron, are also subject to change in this spectrum.

It is possible that varying atmospheric conditions may account for the changes in the spectra of J *Velorum* and 27 *Canis Majoris.* On June 2, 1893, the dark $H\beta$ and $H\gamma$ in J *Velorum* had a fine bright line

superposed. In the spectrum of 27 *Canis Majoris*, bright hydrogen was present in March, 1890, April, 1895 and October, 1897. Numerous photographs of both these spectra on other dates showed all the lines to be wholly dark.

It is evident that a large field of investigation lies open to the spectroscopist among these bright-line stars.

Variable Stars in Large Nebulous Regions: HENRIETTA S. LEAVITT.

Since last March a special study of the distribution of groups of variable stars has been in progress at the Harvard College Observatory. As one result of this investigation, four hundred and fourteen new variables have been discovered and announced. Seventy-three of these are in Orion, one hundred and fifty-two in the Large Magellanic Cloud, fifty-seven in the Small Magellanic Cloud, one hundred and five in Scorpius, ten in Carina and seventeen in Sagittarius. The results of this study up to the present time may be summarized as follows:

First, as regards distribution, it has become evident that groups of variable stars are strongly localized. Of the ninety-nine confirmed variables at present known in the constellation of Orion, south of the equator, eighty-nine are within the limits of Bond's map of the region surrounding the Nebula of Orion, and of these all but four are found in less than half this area. The entire region thus finally limited is nebulous. The large number of variables discovered in the two Magellanic Clouds is in marked contrast with the small number found in the surrounding regions. The neighborhood of the Trifid Nebula in Sagittarius is noticeably poor in variable stars, and so also is the neighborhood of the nebula about η *Carinæ*. Yet these are two of the most densely crowded regions of the Milky Way. In Scorpius, after subtracting thirty-three variables which were found in the cluster Messier 4, there are still left more than four times as many variables as were found in an area in Sagittarius approximately equal in extent and far richer in stars.

Secondly, a certain order of brightness appears, on the whole, to prevail among the variable stars of each group, those in the central condensation of the Large Magellanic Cloud being the faintest, and those in Scorpius the brightest.

Finally, it is probable that different types of variability prevail in different regions. In both of the Magellanic Clouds, a large proportion of the variables appear to have very short periods, while in Scorpius the reverse may prove to be the case. Many of the variables in the Nebula of Orion remain faint during the greater part of the time, but occasionally show a striking increase of brightness. Whether these flashes of brilliancy occur regularly is not yet known.

The researches here described supplement the remarkable discovery by Professor Bailey, of large numbers of variable stars in clusters. They are similar to those carried on by Professor Wolf, of Heidelberg, who has announced lists of new variables in Orion, Aquila and Vulpecula. Evidently a further study of the distribution of groups of variable stars will be intensely interesting in its bearing upon the problems of stellar evolution.

Planetary Spectrograms: PERCIVAL LOWELL.

These spectrograms were made by Mr. V. M. Slipher and the lantern slides of them by Mr. C. O. Lampland, both of the Lowell Observatory staff, and were presented by the director, Professor Lowell.

1. Solar spectrum, photographed November 30, 1903—59 dark lines can easily be counted between G and Hγ.

2. Spectrum of ϵ *Pegasi*, photographed

September 20, 1904—exposure 2^h 15^m. Iron and chromium comparison spectrum. About 59 dark lines can be counted between G and Hγ.

3. Spectrum of Venus, photographed March 9, 1903. This is one of the set from which Mr. Slipher determined the rotation of the planet not to be of twenty-four hours or thereabouts, but very long—iron comparison spectrum.

4. Spectrum of Mars, photographed March 7, 1903—iron comparison spectrum. One of the plates of the set made on Mars by Mr. Slipher to test the measure of precision of the Venus set. The Mars plates gave 25^h $10^m \pm$ for the planet's rotation. The true value is 24^h 37^m. As the precision possible on Mars is only half that possible for Venus the results speak for the decisiveness of the Venus set.

5. Spectrum of Jupiter, photographed November 21, 1903—iron comparison spectrum. The tilt of the lines shows a rotation in 9^h $50^m \pm$, which is exactly the true rotation period as determined by Spots.

6. Spectrum of Saturn, ball and rings, photographed September 7, 1904, on a Cramer 'crown' plate—iron comparison spectrum. The tilt of the lines of the ball in one direction and that of the lines of the rings in the other are well shown, demonstrating that the rings are formed of discrete particles, as proved mathematically by Peirce in part and Clerk-Maxwell in whole and first shown spectroscopically by Keeler.

The spectroscope used in these researches was constructed by Brashear as powerful as possible, especially for the determination spectroscopically of the rotation period of Venus.

The Canals of Mars. An Investigation of Their Objectivity: Percival Lowell.

A new and striking proof of the objectivity of the double canals of Mars has recently come to me in a comparison of the width of the doubles obtained by Schiaparelli in 1888 and by me in 1903. The unintentional character of the corroboration is one of its strongest points. Not only at the time of my observations was his work not in my mind, but not even after the fact had I proposed to compare it.

The following table summarizes the results obtained in 1888 and in the May-June presentation of 1903.

CANALS DRAWN DOUBLE BY SCHIAPARELLI, 1888.

	Times Seen.	Width. °
Euphrates	4	5.1
Phison	4	3.9
Astaboras	3	2.9
Protonilus	4	2.2
Pierus	4	2.4

Canals drawn single by Schiaparelli: Astusapes, Python, Xenius, Rhysius, Apis, Typhon, Hiddekel, Callirrhoe, Deuteronilus.

Two canals, Arnon and Kison, were drawn convergent to the north.

CANALS DRAWN DOUBLE BY LOWELL, 1903.

	Times Seen.	Width. °
Euphrates	11	4.0
Phison	12	3.7
Astaboras	9	3.2
Protonilus	8	2.8
Pierus	2	2.1
Sitacus (faint)	12	3.6

Canals drawn single by Lowell: Astusapes, Python, Rhysius, Aroeris, Cadmus, Ægyptus. Hiddekel generally single, Callirrhoe generally a broad line.

Arnon sometimes convergent to the north, sometimes double. Kison suspicious of convergence to the north.

For both observers the direction of the canal had nothing to do with its single or double appearance.

The conspicuous doubles are the same in the drawings of both observers.

The conspicuous singles are the same in the drawings of both observers.

The Arnon and Kison are convergent in both and in the same direction.

Only the faint or very close doubles show differences at the two presentations.

The double canals, then, declare their own objectivity on three counts, each more compelling than the one before: (1) The fact of showing double, (2) the relative width of the double, (3) the absolute width of the double; and they do this precisely as a real object would, the certainty increasing with the ease of observation. The determination of the absolute width is very difficult, and here we find the probability for reality strong but not expressible; the relative width is easier to determine and the probability for reality is 24 to 1; lastly the determination of the fact of being double, the easiest observation of all, shows the probability that it is real to be 128 to 1.

Note on Three Solar Periods: FRANK H. BIGELOW.

The mean period of rotation of the solar photosphere at the equator is about 26.68 days, as determined by solar observations. There is a mean period of about 25.98 days indicated by terrestrial, magnetic and meteorological observations, which has been regarded as a period of solar rotation. The relative frequency of the solar prominences and the annual variations in the earth's atmosphere show that there is a short cycle of about 1,004 days. These are apparently related together by the equation,

$$\frac{1}{26.68} + \frac{1}{1004} = \frac{1}{25.98} \text{ (approximately).}$$

Some discussion is given of a possible physical cause for this condition, as found in the interior circulation of the sun's mass.

The Coordination of Visual and Photographic Star Magnitudes: JOHN A. PARKHURST.

The importance of stellar photometry among the departments of modern astronomy arises from the fact that the magnitude of a star bears immediately on the star's physical condition and changes. That this is of growing importance is witnessed, among other things, by the numerous discoveries of new variable stars, over three hundred in the present year; showing that variability must be reckoned with as a factor in stellar evolution to an extent that would not have been imagined a decade ago. The relation lately shown to exist between stellar variability and sunspot phenomena adds at once to the interest of the problem and the possibility of its solution.

The photometric catalogues published within the last few years by the Harvard and Potsdam observatories furnish a secure basis for visual photometry, their results agreeing reasonably well except the discordances arising from differences in the star colors. No such basis for photographic magnitudes now exists, therefore to be useful and intelligible, magnitudes must be reduced or reducible to the visual system. But the extension of photometric work demanded by the present needs of astronomy is possible only by photographic means; hence the pressing need of finding some method of harmonizing visual and photographic results for stars differing in type of spectrum and, therefore, in color. The usefulness of such a method will vary somewhat in proportion as it enables us to utilize the photographic magnitudes already obtained.

That the great accuracy of photographic methods applied to the astronomy of position has as yet no counterpart in the astronomy of magnitudes, is due to the one disturbing factor of star color. It is well known that a colored star will affect differently the eye and the photographic plate, but it is not so well appreciated that equal

differences arise in the visual estimates of colored stars by different observers or by the use of telescopes of different apertures. The 'color correction' amounting to one or two magnitudes for a red star in the photograph, is no greater than the difference between the simultaneous Harvard and Rousdon visual estimates of the brightness of such stars. In fact, we find discrepancies of the same kind and similar in amount between different observers, different telescopes, visual and photographic results, and different brands of plates in photography. If the statement is made that no known relation exists between visual and photographic magnitudes, the retort can be made that a normal visual scale does not exist.

The advantages arising from the use of orthochromatic plates have long been recognized, but Scheiner dismisses them with the statement that they can never yield visual magnitudes. The suggestion was first (as far as I am aware) made by Schwarzschild that the difference between the magnitudes of a colored star on ordinary and orthochromatic plates can be taken as a measure of the star's color. If this difference is a function of the color it only remains to find the form of the function, and then complete allowance can be made for the effect of color and that troublesome factor can be eliminated, making possible the reduction of photographic magnitudes to visual, or *vice versa*. Two methods are available for finding the form of the function. First, by trial on known stars of different color (spectral type). To fix our ideas, suppose, for example, that a star of color 5 on Chandler's decimal scale was 7.0 magnitude visually, but photographed 8.0 magnitude on an orthochromatic plate and 9.0 magnitude on an ordinary plate. For such a star the orthochromatic plate gives half the color correction. It is evident that by such experiments with standard stars of known magnitude and color, the form of the function can be found. This work is being done by the writer, under a grant from the Carnegie Institution, using Cramer isochromatic plates in connection with ordinary plates, on the 24-inch reflecting telescope of the Yerkes Observatory. Provisional results thus far obtained are very promising. An independent method which will also be used for finding the form of the function, consists in comparing the intensity curves of the spectra of stars of different types with the intensity curves of the solar spectrum on the two kinds of plates used. It is evident that the photographic effect is the integral of the product of these two curves.

If the objection is urged that the difficulty of coordinating the results obtained with different brands of ordinary and orthochromatic plates, will be equal to the difficulty of harmonizing the visual and photographic systems, it may be met by the suggestion that any brands of plates used should be calibrated by observations of a carefully selected list of standard stars, including each spectral type.

No less important than the choice of plates is the kind of telescope to be used. It seems to the writer that the reflector is the only telescope suited for this work, since by it the rays of all wave-lengths are brought to the same focus.

Emphasis is needed on two further points in regard to the adaptability of the reflector to this work. When extreme ratios of aperture to focal length are avoided, first, the field is very nearly flat; second, the action is very rapid, so that the work can be extended to faint stars. This flatness of field has been denied, both from theoretical reasons and from so-called measures of reflector plates; but it should be stated that the theory is incomplete, not taking proper account of the distribution of light in the 'blurred' image; also that

the measures published by Plummer and Poor were not made on the original negatives, and can not, therefore, be properly called measures. The only real measures so far published, to my knowledge, have been those of the Eros plates taken with the Crossley reflector, and measured at Columbia and Lick. A few of these measures discussed by Hinks showed distortions giving anomalous results near the edge of the plate, but these anomalies are matched on the plates taken at Algiers with the standard photographic refractor, and noticed on the following page of Hinks's paper. It should also be stated that the aperture ratio of the Crossley was large, about 1 to 6.

For this work a diaphragm twelve inches in diameter has been used on the 24-inch reflector, and as the focal length is 93 inches, the ratio is a little greater than 1 to 8. Allowing for the area cut out by the flat, the clear aperture of the mirror is equivalent to $10\frac{1}{2}$ inches. The exposures have been timed to give good measurable images of all the stars on Hagen's charts which extend to twelfth or thirteenth magnitude; in good seeing this requires ten minutes with ordinary plates and fifteen minutes on the isochromatic plates. The magnitudes have been deduced by measurements of disk diameters, the increase per magnitude being nearly uniform and amounting to about 0.025 mm. As the diameters are measurable with a probable error of 0.001 mm., corresponding to 0.04 of a magnitude, the results are comparable with the best visual measures.

This work has some similarity to the spectral photometry of the Draper catalogue, each taking account of the intensity curve of the spectrum; but differs from it in two respects: It is not confined to the bright stars, but can reach to the faintest visible; also, taking account of the entire spectrum, its results will harmonize with visual magnitudes.

The Quadruple System of Alpha Geminorum: HEBER D. CURTIS.

The well-known binary star *α Geminorum* was pronounced by Sir John Herschel to be the largest and finest of the double stars in the northern portion of the sky. Measures, of a very rude character, were made of this pair as early as 1718 by Bradley and Pond, so that this system has been under observation for nearly two hundred years. In spite of this fact some of the elements of the orbit are still quite uncertain, particularly the eccentricity and the period. Values of the eccentricity have been derived, ranging from 0.32 to 0.80, with corresponding periods of 1,001 to 232 years. In recent years the distance between the two components has commenced to decrease, with the result that the elements have become rather more determinate, and Doberck (A. N., 3970) has recently expressed the hope that through this decrease in the distance it will be possible to fix the orbit with considerable accuracy within the next ten or twenty years.

Doberck has derived the following sets of elements, of which he regards the second as the most probable and most in agreement with recent measures.

Elements of Castor.

Ω	29° 29′	33° 56′	42° 34′
λ	84 44	82 26	118 11
i	73 3	63 37	61 56
e	0.7513	0.4409	0.2321
Period	268 years	347 years	502 years
T	1,936.65	1,969.82	1,963.30
a	7″.326	5″.756	6″.467

Retrograde.

In January, 1896, Belopolsky at Pulkova discovered that the fainter of the two stars forming this system is itself a rapid spectroscopic binary.* The period of this

* *Bull. Acad. St. Petersburg*, December, 1896. *Astrophysical Journal*, January, 1897. *Mem. Acad. St. Petersburg*, XI., 4, January, 1900.

component is very well determined, being close to 2.934 days. Belopolsky finds, however, certain irregularities in the observations of successive years which are best explained by the assumption that the line of apsides rotates in about 1,400 days.

Recent spectrograms taken with the remounted Mills spectrograph show that the brighter component is also a spectroscopic binary, and that the system of *α Geminorum* is in reality a quadruple one. Since the discovery of this interesting fact in November of this year a number of plates of both stars have been secured. At present fourteen plates are available for a rough preliminary determination of the period, which seems to be about 27 days. Two early plates of the bright component are rather poor, so that their value in determining the period is somewhat impaired and more plates will be necessary before a more accurate determination of the period can be derived.

Both stars are given in the Draper Catalogue as of type A, and in the later Harvard classification as type VIIIa. H *Gamma* is rather broad and has not been used in the measures. The line at λ 4,481 due to magnesium is very good and there are quite a number of other metallic lines, rather broad and quite faint in the spectrum of the brighter component, and somewhat easier of measurement in the fainter. Helium is apparently absent. There are a number of lines due to titanium and iron, most of the latter being enhanced lines; two lines seem to be due to chromium. With proper exposure (about sixteen minutes) from fifteen to twenty-five measurable lines are found. The total range in the radial velocity is about twenty-one kilometers, and the preliminary determination of the velocity of the center of mass of the system is approximately + 5 km. per second. The corresponding constant for the fainter component is given by Belopolsky in his latest paper as — 4.1 km. per second. It is well known that where the elements of the visual orbit of a binary and the relative radial velocities of its components are both known it is possible to derive an accurate value of its parallax. Assuming the relative radial velocity to be nine kilometers and using the period of 347 years and the corresponding elements which Doberck regards as the most probable, we find a parallax of 0″.03. Using the other orbits given above, however, we should get values differing widely from this. It is evident that such results are meaningless till the elements of the visual orbit are known more definitely.

In the star *α Geminorum* we have a well-established quadruple system, and it is hoped that the more detailed investigation which the writer has in progress will give a definite determination of the relative radial velocity of the two systems, so that with the improvement of the visual orbit we may in time have a relatively very exact knowledge of the distance, mass and orbital dimensions of this complex star.

Use of the Method of Least Squares to decide between Conflicting Hypotheses: Harold Jacoby.

In 1901 the writer published* a theorem concerning the application of least squares when it is necessary to choose between two different methods of reducing observations. The theorem was doubtless well known, but the writer was unable to find it in print. Since then, Mr. Midzuhara, of the Tokyo Astronomical Observatory, has written three interesting articles† in which, among other things, he gives a different proof of the writer's theorem, and also obtains another analogous one. The object of the present note is to point out a very important divergence between Mr. Midzu-

* *Astr. Jour.*, 514.

† *Astr. Jour.*, 521, 535, 568.

hara's conclusions and the writer's; and also to show how one of the former's most interesting results can be obtained in a manner different from that used by him.

The writer's theorem is: "Let there be given two series of observation equations as follows:

$$\left.\begin{array}{l} a_1x + b_1y + c_1z + \dots + n_1 = 0, \\ a_2x + b_2y + c_2z + \dots + n_2 = 0, \end{array}\right\} (1)$$
$$\vdots \qquad \vdots$$
$$\left.\begin{array}{l} a_1x + b_1y + c_1z + \dots + p_1w + \dots + n_1 = 0, \\ a_2x + b_2y + c_2z + \dots + p_2w + \dots + n_2 = 0, \end{array}\right\} (2)$$
$$\vdots \qquad \vdots$$

the equations being identical in the two series except for the addition of one or more new unknowns $w, \cdots$ in (2). Let each of these series of equations be solved by the method of least squares, and let: $[vv]_1$, be the sum of the squares of the residuals resulting from the solution of equations (1); $[vv]_2$, be the sum of the squares of the residuals resulting from the solution of equations (2); then, no matter what may be the law of the coefficients $p_1, p_2, \cdots$, and even if these coefficients are assigned at random, $[vv]_1$, is always larger than $[vv]_2$."

The conclusion drawn by the writer from this theorem is as follows:

"The method of least squares is used ordinarily to adjust series of observation equations so as to obtain the most probable values of the unknowns. But there is a subtler, and perhaps more important use of the method; when it is employed to decide which of two hypothetical theories has the greater probability of really being a law of nature; or to decide between two methods of reducing observations. In such cases, astronomers not infrequently give preference to the solution which brings out the smallest value of $[vv]$, the sum of the squared residuals. But in the light of the above theorem, it becomes clear that the mere diminution of $[vv]$ alone is insufficient to decide between two solutions, when one involves more unknowns than the other. To give preference to the second solution, it is necessary that the diminution of $[vv]$ be quite large, and that the additional unknowns possess a decided *a priori* probability of having a real existence."

In his paper in *Astr. Jour.*, 521, Mr. Midzuhara says: "This conclusion, perhaps, depends on the author's misapprehension of the principle of probability. For I believe that to compare the probabilities of the two solutions we must necessarily take

$$\frac{[vv]_1}{m-\mu_1} \text{ and } \frac{[vv]_2}{m-\mu_2}$$

where m expresses the number of observations, and μ_1 and μ_2 are the numbers of the unknown quantities in the first and second solutions, respectively."

In other words, Mr. Midzuhara takes as the criterion for deciding between the two solutions the quantity ordinarily called 'mean error of one equation,' instead of the sum of the squared residuals. When the number of unknowns in the two solutions is different, these two criteria may give opposite results; the one indicating the first solutions as the more probable, the other, the second solution.

It is evident that practise of astronomers varies in this matter. Mr. Midzuhara, for instance, and doubtless other astronomers, too, use $[vv]/m - \mu$ as the criterion. On the other hand, Bessel was in the habit of using $[vv]$. A good example is to be found in his classic paper on the parallax of 61 *Cygni*. He there* reduces his observations with parallax terms, and again without them. He decides in favor of the reality of his parallax terms solely on account of the diminution of $[vv]$; and not until after this is decided does he compute the mean error $\sqrt{[vv]/m - \mu}$. This quantity he calculates for the parallax solution only,

* *Astr. Nach.*, No. 366, p. 87.

not for both solutions. So far is he from using it as a criterion.

It would, indeed, appear that very simple reasoning indicates $[vv]$ as the right criterion. If we consider observation equations of the general form:

$$\phi(x, y, z, \ldots, n,) = 0,$$

the ordinary solution determines $x, y, z, \cdots$, so as to make $[vv]$ a minimum. If there exists a doubt as to whether the form of the function ϕ should be either ϕ_1 or ϕ_2, this fact simply transfers ϕ to the list of unknowns, and we must so determine $\phi, x, y, z, \cdots$, as to make $[vv]$ a minimum. We shall do this if we make two ordinary least squares solutions for ϕ_1 and ϕ_2, the only possible values of ϕ, and prefer that solution which gives the smaller $[vv]$. Since the other criterion may give an opposite result, that other criterion must be wrong.

It may be of interest to add to the above a remark concerning the attractive result obtained by Mr. Midzuhara in his equation (13).* This result is:

$$[vv]_1 - [vv]_2 = w^2 P_w, \qquad (13)$$

where w is the value of the new unknown, obtained in the solution of our equations (2) and P_w its weight from the same solution. Mr. Midzuhara gives a somewhat extended demonstration of this equation (13); it may, however, be obtained almost directly from a principle demonstrated by Gauss in 'Elementis Ellipticis Palladis.'† It is there shown that if μ be the number of unknowns, and if the normal equations are solved by the Gaussian method of elimination:

$$[vv] = [nn \cdot \mu],$$

where $[nn \cdot \mu]$ denotes the usual Gaussian auxiliary. In the present case, if there are μ unknowns in equations (1), and $\mu + 1$ in equations (2), we shall have, at the end of our Gaussian elimination:

$$[pp \cdot \mu]w + [pn \cdot \mu] = 0,$$
$$[nn \cdot \mu]$$
$$[nn \cdot (\mu + 1)].$$

But, according to Gauss's principle:

$$[nn \cdot \mu] = [vv]_1,$$
$$[nn \cdot (\mu + 1)] = [vv]_2,$$

and, as usual:

$$[nn \cdot (\mu + 1)] = [nn \cdot \mu] - \frac{[pn \cdot \mu]^2}{[pp \cdot \mu]}.$$

Therefore:

$$[vv]_1 - [vv]_2 = \frac{[pn \cdot \mu]^2}{[pp \cdot \mu]}.$$

But:

$$w = -\frac{[pn \cdot \mu]}{[pp \cdot \mu]},$$

and:

$$[pp \cdot \mu] = P_w = \text{weight of } w,$$

so that:

$$[vv]_1 - [vv]_2 = w^2 P_w.$$

This is Mr. Midzuhara's equation (13).

Tables for the Reduction of Astronomical Photographs: HAROLD JACOBY.

In 1895 the writer published a paper entitled 'On the Reduction of Stellar Photographs, with Special Reference to the Astro-Photographic Catalogue Plates.'* As indicated in the title, the method there described was intended primarily for the reduction of large series of plates made at the same declination. But ordinary stellar photographs intended for star-cluster catalogues, solar or stellar parallax, etc., usually involve so few plates of a single declination that it is not economical to prepare the kind of special tables suitable for a photographic catalogue of the whole heavens. Moreover, Contribution 10 of the Columbia Observatory has long been out of print, so that it is now impossible to supply copies to those asking for them.

* *Astr. Jour.*, 568.

† 'Werke,' Vol. 6, p. 22.

* 'Contrib. from the Obs. of Columbia Coll.' No. 10; and in French, *Bull. Com. Perm.*, Tome III.

For these reasons, the writer has prepared the present modification of his method, and has added tables suitable for the reduction of isolated groups of plates made at any declination distant more than 15° from the pole.

The tables will appear in a short time as one of the Columbia 'Contributions.'

Recent Researches of the Henry Draper Memorial: EDWARD C. PICKERING.

A photograph was shown of the spectrum of λ *Cephei,* which has a spectrum closely resembling that of ζ *Puppis* and contains the second series of lines probably due to hydrogen. A method of observing occultations photographically was explained and a printed enlargement of a photograph of the emersion of η *Virginis,* on December 28, 1904, was shown, which had been taken by Mr. Edward S. King. A rotary motion was given to the plate-holder so that the star gave a continuous trail, the time being indicated by a motion given to the plate at regular intervals. It appeared that the star increased in light during emersion for about a third of a second. The principal portion of the paper was devoted to the study of the distribution of the stars according to their spectra. The results were based upon an examination of the Draper plates by Mrs. Fleming. About 6,000 plates have been inspected, each showing on the average the spectra of a thousand stars, with small dispersion, and on these all that were peculiar were noted. Using a larger dispersion, about thirty thousand stars have been classified and catalogued. Visual counts of the number of stars in different parts of the sky have little value owing to the uncertainty of the magnitudes. The same might be said of a classification of photometric magnitudes of the stars taken as a whole. It was shown that stars of each class of spectrum should be considered by themselves, as the distribution differs widely. Thus, the Milky Way appears to consist wholly of stars of the first type. The helium or Orion stars have a different distribution, forming a Milky Way of their own, mainly in Orion and Argus. It was shown that the classification of the variable stars, proposed by the writer in 1880, was confirmed by their spectra, and that the latter formed a means of determining the class, in some cases, even better than the light curve.

Note on Two Variable Star Catalogues: J. G. HAGEN.

Father Hagen presented to the meeting some specimen pages of two Catalogues of Variable Stars, now in preparation, one by the Astronomische Gesellschaft and the other by Professor E. C. Pickering. Of the former catalogue seven pages had been printed for presentation at the astronomical congress in Lund, last September. A copy had been sent to Father Hagen in time for the Philadelphia meeting, but Dr. Mueller's report at Lund came, unfortunately, too late. For this reason only those features of the catalogue could be mentioned that presented themselves to the reader of these seven pages.

More definite explanations could be given on the other catalogue, since Professor Pickering was himself present, and had shown one specimen page previously to several friends. His catalogue will be a 'Bibliography of the Variable Stars,' with the lists of the known maxima and minima, and the sources from which they were taken. Father Hagen brought out the fact that the two catalogues will supplement one another. The catalogue of the Astronomische Gesellschaft will give exact positions and elements of light variations, with very condensed references to all accessible publications, on each variable star. Professor Pickering's 'Bibliography' will give fuller details of the spectra of the variables stars from the rich material at

the Harvard College Observatory, and will put the lists of known maxima and minima in the convenient shape of tables. While the former catalogue will make a volume of about 500 pages (quarto size), the latter will only have one third of this bulk. Both catalogues will be a very valuable accession to our literature on this subject.

Useful Work for a Small Equatorial—A Proposed Discussion.

The discussion was opened by Professor Edward C. Pickering. He stated that measurements of wide double stars might be useful, but that the positions of stars much more than 5″ apart could be better determined by photography, while closer stars required a large telescope. The brightness of stars can now be readily and inexpensively determined with a wedge photometer, and the relative light of the components of close doubles by a polarizing photometer. The Herschel-Argelander method could be usefully applied to faint stars, especially to the components of coarse clusters, and to Durchmusterung zones, inserting all stars brighter than a fixed magnitude. Variable stars of long period can be usefully followed by inexperienced observers, since the range is large. Observations of suspected variables, of Algol, and other short period variables, are likely to be of little value, except when made by observers having long experience. But little useful work could be done with spectroscopes attached to small telescopes. A search for new stars in the Milky Way, and an examination of known nebulæ to see if they are gaseous, as was, perhaps, first done by Col. John Herschel, might prove of value. Observations of Jupiter's satellites, comets, sunspots and solar prominences were also mentioned as useful fields of work for instruments of this class.

FRANK B. LITTELL,
For the Council.

SCIENTIFIC BOOKS.

Die Moore der Schweiz, mit Berücksichtigung der gesamten Moorfrage. Von Dr. J. FRÜH und Dr. C. SCHRÖTER. (Beiträge zur Geologie der Schweiz, herausgegeben von der geologischen Kommission der Schweiz, naturforschenden Gesellschaft, geotechnische Serie, III Lieferung.) Bern, 1904. 4°, pp. xviii + 751. 45 text-cuts, 4 plates and a map.

Probably every person seriously interested in peat-bogs (or, as we may better call them, peat-moors), whether it be from a geological, a phyto-ecological or an economic standpoint, has known that the present work was in preparation and has eagerly anticipated its appearance. The authors are well known as among the foremost authorities upon the subject, and their work now before us fully satisfies our high expectations. While primarily devoted to the study of the Swiss moors, the authors nevertheless discuss every question also from the general or world standpoint, so that the work as a whole is in reality a study of peat-moors based upon those of Switzerland as types. It is divided into two parts, a first devoted to Moor-questions in general (435 pages), and a second given to a systematic description of those of Switzerland (310 pages). Under the first part is discussed, the general nature and place of moors, peat-building plant-groups (a modern ecological study), peat and its nature, geology of moors, geographical distribution of moors, a geomorphological classification of the moors of the world, nomenclature in relation to physical features, agricultural conditions of the Swiss moors, post-glacial vegetation history and its reconstruction through moors. Every chapter is characterized by exhaustive but clear treatment, by copious citation of literature, including that of this country, and by appropriate illustration. Among the illustrations are many of those diagrammatic vegetation cuts now coming into vogue in ecological works, while the plates include two typical photographic moor-scenes, of which we could wish there were many more. It is impossible here to particularize farther, and it must suffice to say that this work is incomparably the

most exhaustive, authoritative and generally excellent treatise upon its subject which has yet appeared, and that it must form the foundation-work for all future studies upon matters connected with peat-moors.

W. F. GANONG.

SCIENTIFIC JOURNALS AND ARTICLES.

THE January number of the *Botanical Gazette* contains a paper by Rodney H. True and C. S. Oglevee giving the results of studies on the effect of such insoluble substances as sand, starch grains, filter paper, etc., upon the toxic action of electrolyte and non-electrolyte poisons in aqueous solution. It appears that the insoluble body adsorbs the poison solute, thus diminishing the effective concentration of the latter as though it were taken out of solution.—Burton E. Livingston describes the types of soil and of vegetation in the north-central part of the southern peninsula of Michigan (Roscommon and Crawford counties), and discusses the influence which the soil has in determining the distribution of the various plant societies. He concludes that the amount of soil moisture, determined largely by fineness of soil particles, is the main controlling factor here.—A. D. E. Elmer describes a number of new and noteworthy Californian plants.—Edgar W. Olive discusses the morphology of *Monascus purpureus*, taking up the accounts of Barker and Ikeno and giving certain results of his own observations.—B. M. Davis discusses fertilization in Saprolegniales based upon a recent paper by Trow; and also the sexual organs and sporophyte of Rhodophyceæ based upon a recent paper by Wolfe.

The Journal of Nervous and Mental Disease for January opens with a paper by Dr. F. X. Dercum, giving an exhaustive and careful report of three cases, one being illustrated, which bear upon the question of the relation of syphilis to spastic spinal paralysis and also indirectly upon the question of Erb's form of spinal syphilis. Dr. S. D. Ludlum contributes an article on the 'Possible Relationship of Neuro-fibrillar Changes to Insanity.' He summarizes the literature bearing on the subject, and reports a series of experiments conducted at Friends' Asylum which leads to the hypothesis of a close relationship between fibrils and mental manifestations. An interesting case of tumor of the occipital lobe with an unusual clinical history is reported by Dr. Philip Zenner; also one of carcinoma of the spine following carcinoma of the breast, the spinal disease being characterized by a phenomenally long course, possibly due to removal of the ovaries some four years before the patient's death. The October meetings of the New York Neurological Society and the Boston Society of Psychiatry and Neurology are reported. The 'Periscope' for the month contains abstracts of the following journals: *Monatsschrift für Psychiatrie und Neurologie, Brain, Neurologisches Centralblatt, Revue de Psychiatrie et de Psychologie Expérimentale, Centralblatt für Nervenheilkunde und Psychiatrie, American Journal of Insanity, Journal de Neurologie, Archives de Neurologie,* and selected articles from miscellaneous periodicals. The books reviewed in this number are 'Epilepsy and its Treatment,' by Dr. W. P. Spratling; 'La Mimica del Pensiero Studi e Ricerche,' by Dr. Sante de Sanctis; two volumes of 'The Doctor's Recreation Series,' edited by C. W. Moulton; 'Manuel pour l'Etude des Maladies du Système Nerveux,' by Dr. Maurice de Fleury; 'A Manual of Psychology,' by G. F. Stout; 'Trattato delle Malattie Mentali,' by Professor E. Tanzi; 'Lehrbuch der Nervenkrankheiten für Aerzte und Studierende,' by Professor H. Oppenheim; 'Essentials of Nervous Diseases and Insanity,' by Dr. J. C. Shaw; 'Nietzsche,' by P. J. Möbius; 'Mental Defectives, their History, Treatment and Training,' by Dr. M. W. Barr, and 'The Physician's Visiting List for 1905–1906.' The issue closes with two pages of 'News and Notes.'

SOCIETIES AND ACADEMIES.

THE NEW YORK ACADEMY OF SCIENCES. SECTION OF GEOLOGY AND MINERALOGY.

AT the meeting of the section held on February 6 the following papers were read by title:

Moissanite, a Carbon Silicide from the Cañon Diablo Meteorite: GEORGE F. KUNZ.

On Zirkon from Lawton, Oklahoma: George F. Kunz.

On Monazite Sand from Idaho: George F. Kunz.

A paper entitled 'The Serpentine and Associated Asbestus Minerals of Belvidere Mountain, Vermont,' was presented by V. F. Marsters, of Columbia University.

Belvidere Mountain lies approximately along the line between the counties of Orleans, Lamoille and Franklin. It is a sharp crested ridge with a maximum elevation of some 2,100 feet above Eden Corners at its southern termination. Three topographic elements are prominent, a sharp crested ridge forming the upper 900 feet of the mountain, a crescentic plateau with a flat top 1,200 feet above the valley floor and rimming the end of the mountain, and lastly a steep lower slope composing the foot of the plateau and extending to the valley bottom.

The upper part with steep slopes is composed of amphibolite. In addition to the hornblende which makes up seventy-five per cent. of the rock, there is also present an inconsiderable amount of epidote and a non-pleochroic colorless mineral regarded as zoisite, together with magnetite and pyrite. Towards the base, garnet becomes a prominent constituent, sufficient to make a well-defined garnet zone. In nearly all cases observed, the garnet is largely altered to penninite, a variety of chlorite. Along the garnet zone the hornblende has also undergone marked alteration in part to serpentine. The nose-like projection forming the plateau is composed of serpentine. In this rock occur the so-called asbestus deposits recently prospected and worked for this product. In thin sections the serpentine appears to be made up largely of a felty and fibrous mass, apparent only under cross nicols. It is typical fibrous serpentine. In thin sections from the upper part of the plateau and in close proximity to the overlying amphibolite, there appear shredded masses presenting the original structure of hornblende as seen in the amphibolite, but mineralogically altered to a fibrous mass with the optical characteristics of anthophyllite. It is not improbable, moreover, that a portion of the hornblende has altered to tremolite. These fibrous constituents form the so-called 'slip-fiber.'

The serpentine belt has also been subjected to peculiar faulting and crushing. The cracks thus produced, even on a microscopic scale, have been filled with these fibrous constituents, and then the whole mass submitted to further slipping. This has caused the slickensiding phenomena on the fracture planes and a consequent stretching of the fibrous content; hence the term 'slip-fiber.' 'Cross-fiber' or true chrysotile is to be found in this area. It is best developed along lines of maximum fracture and minimum lateral thrust. There appears to be two bands of maximum fracture, one extending along the upper portion of the plateau and not far from the garnet zone, the second along the foot of the plateau and best shown on the property of Judge Tucker.

The next paper was by Dr. Charles P. Berkey, on the 'Interpretation of Certain Laminated Clays with their Bearing upon Estimates of Geologic Time.'

Laminated clays of glacial and post-glacial age are abundant in many districts of the northern states and Canada. They are especially abundant about the head of Lake Superior where their origin is intimately related to the closing fluctuations and final withdrawal of the Wisconsin ice sheet.

One of these deposits, at Grantsburg, Wis., exhibits a remarkable uniformity of structure and is so clearly bounded by other accumulations of known significance that its history is readily interpreted. From a detailed analysis of the laminated structure it is argued that this deposit was about 1,700 years in accumulating.

A like interpretation of the similar isolated deposits following the retreating ice sheet would give data for time estimates from an entirely new standpoint. In some areas laminated clays occupy interglacial position and it may be possible to apply the same method to them.

The last paper of the evening was by Professor A. W. Grabau, on the 'Evolution of Some Devonic Spirifers.' *Spirifer mucronatus* (Conrad) is a Linnæan species comprising a large number of mutations. A remarkable fact is

that all mutations pass through a mucronate stage such as is characteristic of the adult mutation after which the species is named. (The term *mutation* is here used in the sense in which it was originally proposed by Waagen, and not in that in which it was subsequently used by de Vries, *i. e.*, for the result and not for the process.) A still earlier stage in development (nepionic) shows the non-mucronate features of the ancestral species similar to *S. duodenarius* of the Onondaga. The mucronate feature is carried to excess in a number of mutations of the Lower Hamilton group. It is especially persistent in the Michigan region. This type of outline is accompanied by a rib in the median sinus and a depression in the fold. In Ontario the primitive mucronate type gives rise upward to a number of mutations which are especially characterized by progressive increase in height without corresponding lengthening of the hinge. The median plication and depression quickly disappear.

Acceleration and retardation in development are the chief principles which explain the development of the great number of mutations. For the principle of retardation the term ***bradygenesis*** (from βραδύς, slow) was proposed, corresponding to the term *tachygenesis* proposed by Hyatt for acceleration.

In the New York province the primitive mucronate type gives rise to high and short-hinged mutations, but these retain the median rib and depression. In form these are tachygenetic; in respect to the surface features, bradygenetic. In the arenaceous beds of the later Hamilton in eastern New York, a mutation with many ribs and moderate mucronations exists. This is in many respects a bradygenetic type. Side by side with extremely accelerated or tachygenetic types in all horizons (*i. e.*, very short-hinged, non-mucronate, high and thick mutations) occur slightly retarded or bradygenetic types which retain in the adult the mucronate character which is typical of the young of all the mutations.

A. W. GRABAU,
Secretary.

COLUMBIA UNIVERSITY.

THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

THE 596th meeting was held February 4, 1905.

Mr. J. F. Hayford, chief of the Computing Division of the Coast and Geodetic Survey, reported briefly on the completion of the reductions of the leveling between Seattle and several points on the Atlantic coast; the apparent difference of level between the two oceans is 187 mm., 0.6 foot. The levels run at Nicaragua gave a difference of zero.

Mr. Bernard R. Green then spoke on 'Public Buildings in Washington,' presenting many lantern views, and outlining the problems that have been forced on his attention during the many years he has been connected with the erection of such buildings. Attention was called to the peculiar far-sighted plan of the city, with its two centers at the Capitol and White House, marked by monumental buildings. The majority of the government buildings, he held, should be expressive of their use and so be of the workshop or office type rather than monumental, and should be well scattered. The future buildings of the monumental class will probably be of the modified classical type, massive, of stone, and relatively low, *e. g.*, eighty feet. The cost of these is high; the State, War and Navy Building cost $1.06 per cubic foot, the Library of Congress sixty-three cents, including decorations. Buildings of the other class may be of the columnar type, a steel skeleton structure which carries the outer walls, and costs twenty-five to thirty cents per cubic foot.

Mr. S. W. Stratton, director of the Bureau of Standards, then spoke on 'The Architectural and Engineering Features of Scientific Laboratories,' exhibiting many views of such buildings and especially the new ones of the Bureau of Standards. The most important conditions to be secured are freedom from vibration and jars, and perfect control of the ventilation and temperature of the rooms. An excavated basement is a great source of trouble; the heating and power-plants and machinery should be in a separate building. Substantial reinforced concrete floors render piers unnecessary in the first story, while in the second story wall-brackets are generally better

than floor supports for apparatus. Numerous other details were considered.

The subject of stability was further discussed by several of the physicists and astronomers present.

CHARLES K. WEAD,
Secretary.

DISCUSSION AND CORRESPONDENCE.

RECENT WASHINGTON RHIZOBIA EXPERIMENTS.*

IN 1902 Dr. Geo T. Moore published a paper in which he gave a brief outline of the history of the study of the free nitrogen-assimilating microbes of leguminous plants.† In this paper the author outlines a method for increasing the nitrogen-assimilating power of rhizobia by growing them upon artificial nitrogen-free media, which is said also greatly to increase their tubercle-forming power. According to the paper by Grosvenor, Dr. Moore has continued his experiments along the same line and has patented the process, giving the patent rights over to the government for the sole benefit of the farmer. It is stated that by the use of these nitrogen-hungry rhizobia the yield of any leguminous crop may be increased very greatly (from 40 to 400 per cent.). The results are said to be far superior to those obtainable from the use of the 'Nitragin,' patented by Nobbe and Hiltner of Germany. Instead of bottling the cultures (of nitrogen-hungry rhizobia) in a dry pulverulent state, as did Nobbe and Hiltner, Dr. Moore infiltrates absorbent cotton with the cultures and dries it, whereupon it is ready for shipment to the farmer, at a nominal cost.

If the claims of the paper can be verified by further tests, Dr. Moore deserves credit for having accomplished a work which will prove to be of great benefit to farmers. It will of course not do away with the necessity of crop rotation.

It is regrettable that Dr. Moore did not see fit to contribute the article himself and that it did not appear in some scientific publication rather than a literary magazine. This is not at all intended as a criticism of Mr. Grosvenor's presentation of the work done by Dr. Moore, only the custom prevails for those who do the actual scientific work to also present it to the world first-hand, nor are we in the habit of looking for reports of research work in publications devoted almost wholly to fiction.

ALBERT SCHNEIDER.

* Gilbert H. Grosvenor, 'Inoculating the Ground: A Remarkable Discovery in Scientific Agriculture,' *The Century Magazine*, **68**: 831–839 (October), 1904.

† Geo. T. Moore, 'Bacteria and the Nitrogen Problem,' Year-book of the Department of Agriculture, pp. 333–342, 1902.

SPECIAL ARTICLES.

A NEW CODE OF NOMENCLATURE.

IN *The Condor* for January, 1905 (Vol. VII., pp. 28–30), is an abstract of a new code of nomenclature, "which will shortly appear under the joint authorship of Doctors Jordan, Evermann and Gilbert, * * * entitled 'Nomenclature in Ichthyology. A Provisional Code Based on the Code of the American Ornithologists' Union.'" It is said:

> The recent preparation of numerous papers in systematic ichthyology has necessitated the reconsideration of many problems of zoological nomenclature, and as some of these are not covered by any canon in any recognized code, and again, as certain canons in the best considered of the various codes of nomenclature, that of the American Ornithologists' Union, are not available in the study of fishes, we have ventured to draw up a code for our own use in ichthyology. * * * The different canons in this code are based on those composing the code of the American Ornithologists' Union, and so far as possible the language of that admirable document has been followed. We have, however, omitted certain matters which may be considered as self-evident, and we have omitted all reference to groups of higher than family rank.

The points in which the ichthyological code differs from the ornithological are then stated; the text of these parts of the new code is given apparently in full, and relates to six of the canons of the earlier code. As the perfect code has not as yet been devised, all improvements on preceding codes should, of course, be welcomed, but changes from well-established methods of procedure should carry convincing evidence that they are improvements in order to secure adoption.

Not many months ago the American

botanists issued a new 'Code of Botanical Nomenclature,'* they having found the Paris Code of 1867 out of date and unsatisfactory. This code does not depart essentially from the A. O. U. Code, but on some points it is fuller and more explicit, and at the same time more concise. As said in another connection:

> The A. O. U. Code was a pioneer in innovations which have now become very generally accepted, but which then (1886) required argument and extended illustration. * * * Provision is made for a few points not covered by the A. O. U. Code, but the spirit and principles of this code are * * * closely followed. * * * †

In the Ichthyological Code the new rulings principally relate: (1) To competitive specific names published simultaneously; (2) to competitive generic names published simultaneously; (3) to the determination of a generic type in cases where no type has been indicated by the author; (4) to the admissibility of orthographic variants of generic names.

The primary purpose of all codes of nomenclature is stability of names; how best to accomplish this under complicated conditions is still an open question. The points on which leading authorities still differ are mainly those above stated, and respecting which a new departure is proposed. These may be taken up briefly in sequence.

1. *Specific Names Published Simultaneously.*—"Canon VI. Of competitive names otherwise tenable, given by the *same*‡ author, that one is to be preferred which stands first in the text. In case of competitive names otherwise tenable, given by *different* authors of the same actual date, so far as ascertainable, the one standing on the earlier page of *its publication* must be chosen."

To this ruling there is no objection, provided authors will uniformly adhere to it. This method was considered in framing the A. O. U. Code, but was deemed too arbitrary, as the author publishing a large book, with new names introduced in the middle portion or toward the end, would have no chance against the man publishing new names in a small book or in a short pamphlet, however superior his accompanying diagnoses might be. For this reason the A. O. U. Code (Canon XVII.) proposed alternatives, perhaps better applicable in ornithology than in some other branches of zoology. Thus preference is to be given, first, to the name founded on the male to that founded on the female; second, to that founded on the adult to that founded on the young; third, to that founded on the nuptial condition to that on the pre- or post-nuptial condition.

2. *Generic Names Published Simultaneously.*—"Canon VII. In case of competitive generic names otherwise tenable, published in the same work, preference shall be given to the one standing first in the work. Of competitive generic names of the same actual or ostensible date (no exact date being ascertainable) given by *different authors,* that one is to be taken which is proposed on the *earlier page of the volume in which it appears.* When the same generic name is given to two distinct genera of animals at the same date (as far as ascertainable), the name appearing on the earlier page shall be deemed to have precedence."

Here again the ruling is rigidly arbitrary as between earlier and later pagination in *different publications.* The A. O. U. Code (Canon XVIII.) provides, under such contingencies, that: "1. A name accompanied by the specification of a type takes precedence over a name unaccompanied by such specification. 2. If all, or none, of the genera have types indicated, that generic name takes precedence the diagnosis of which is most pertinent." Here comes in the element of personal decision as against arbitrary rule, but the cases are extremely few where the proper course of action is not evident.

3. *The Determination of Generic Types.*—Canon X. of the Ichthyological Code relates to the fixing of the type of a genus, when no type has been indicated by the author. On no nomenclatorial question is there greater

* *Bulletin Torrey Botanical Club,* Vol. XXXI., No. 5, May, 1904, pp. 249–290.

† *Auk,* Vol. XXI., July, 1904, pp. 404, 405.

‡ The italics in these quotations are not in the original, but are used here to draw attention to special points.

diversity of usage or greater strenuosity of opinion than on this, although the tendency is, or was formerly, to follow one of two courses, either to take the first species as the type, or to determine the type by the principle of elimination, under certain reasonable restrictions. Of late the latter has been the course favored by the greater part of those systematists who have any special regard for rules of nomenclature. Two qualifications of the strict rule of determining the type by elimination have been widely accepted. One is that when a genus containing a number of species is divided, and the name of one of the species is chosen as the name of a new genus, the type of that genus shall be the species the name of which has been selected as the name of the genus—a perfectly logical, unequivocal proceeding, open to no reasonable objection.

A second exception is that of the A. O. U. Code, which provides that if a "genus contains both exotic and non-exotic species—from the standpoint of the original author—and the generic term is one originally applied by the ancient Greeks and Romans, the process of elimination is to be restricted to the non-exotic species." In this way the name is retained in nearly its ancient sense, and its transference to an irrelevant association is prevented. This exception comes in mainly, of course, in connection with Linnæan and Brissonian names, and is akin to that other rule, more or less tacitly held in the minds of many systematists, that the type of a Linnæan genus should be the best known European or officinal species originally included within it.

Canon XXI. of the A. O. U. Code is: "When no type is clearly indicated the author who first subdivides a genus may restrict the original name to such part of it as he may judge advisable, and such assignment shall not be subject to subsequent modification." This was not a new rule when announced by the A. O. U. in 1886, but was a part of the British Association Code originally promulgated in 1842, and reaffirmed by nearly every later code down to 1905, when three revolutionary ichthyologists came forward with the following as their Canon X.: "The type of a genus can be indicated by the original author only. * * * In every case, the determination of the type of a genus shall rest on evidence offered by the original author, and shall be in no wise affected by restrictions or modifications of the genus in question introduced by subsequent authors, nor shall the views or the dates of subsequent authors be considered as affecting the assignment of the type of a genus"! For such a reactionary and far-reaching proposition there should certainly be most convincing and satisfactory reasons, for it involves the overthrow of the consistent usage of the majority of systematists for the last half century, and invites at least temporary chaos in the place of what seemed permanent stability. The proposed new ruling should leave nothing to personal opinion, but should provide a rule of unquestionable applicability to all cases.

The argument for the new proposition is as follows: "It is believed that the principle that a generic name must be fixed by its original author is one of vital importance in nomenclature. All processes of fixing types by elimination or by any other resting on subsequent literature, lead only to confusion and to the frittering of time on irrelevant questions. The method of elimination can not be so defined as to lead to constant results in different hands. In general it is much more difficult to know to what types subsequent authors have restricted any name *than to know what the original author would have chosen as his type.* Most early writers who have dealt with Linnæan species have consciously or unconsciously encroached on the Linnæan groups rather than made definite restrictions in the meaning of the generic names."

In determining types and the tenability of names it is notorious that the systematist is and must be guided by what an author has done and not by what he may have intended to do, no matter how evident the unaccomplished intention may be. Rules, to be effective, must be rigidly enforced, regardless of personal preference in favor of some particular result. But the foregoing is a proposition to override rules and usages that have brought nomenclature to a reasonable condition of stability respecting a wide class of cases it is

now proposed to reopen and subject to a new decision based largely on personal caprice. How is thus stated: "This may be done by direct statement [on the part of the author] that a certain species is a type species [a statement at present always respected and welcomed], the leading species, the 'chef de file,' or by other phraseology conveying the same idea [information always welcomed and in these days earnestly searched for and regarded]; it may be indicated by the choice of a Linnæan or other specific name as the name of a genus [also, as said above, recognized as a guiding principle], or by some statement which shall clearly *indicate an idea in the author's mind* corresponding in fact, if not in name, to the modern conception of the type of a genus. [Here, unfortunately, is the loophole for diversity of opinion as to whether the author had such an idea, and, if so, which of several species best meets the author's unexpressed conception. The decision of one author, in many instances, is likely, in the nature of the case, to be different from that of another, and the firm ground absolutely necessary for the proposed revolutionary procedure is wanting. Finally,] The type of a Linnæan genus must be, in the phraseology attributed to Linnæus, 'the best known European or officinal species,' included by that author within the genus [—an injunction already in force]."

We have here then several sound principles, which are not new but already in force, and a new proposition to enable an author who is in too much of a hurry or too indolent to find out what other authors have done under the principle of elimination toward fixing the type of a genus not otherwise determined, to fix the type offhand for himself on the basis of his own conception of what the author's idea was as to the type of his group, when, in a large proportion of cases, the author almost unquestionably never gave the matter a thought, or even entertained the idea of a type in the modern sense. What he may have thought is, in most cases, purely a matter of guesswork.

It is not quite true, as said in the new ichthyological code, that 'the method of elimination can not be so defined as to lead to constant results in different hands.' The results will vary somewhat with the experience and qualifications of the user of the method, if the conditions of the question are especially complicated and perplexing; but my experience has been that experts in such cases rarely reach different conclusions, especially if they are able to confer and discuss the case.

Canon XI. of the new code is in line with Canon X. It reads: "In case a genus requiring subdivision or modification contains as originally formed more than one species, and the author of the genus does not in any way clearly indicate the type, the first species named in the text by the author as certainly belonging to the genus shall be considered as its type." The enforcement of this rule would obviously, in some instances at least, lead to the gratuitous displacement of generic names which have long since reached a stable equilibrium under the principle of the determination of the generic type by elimination—the disturbance of simple cases universally accepted as settled, and, therefore, a well nigh wanton proceeding.

4. *The Recognition of Variants of Generic Names.*—Modern codes of nomenclature are practically unanimous in ruling that a generic name is untenable 'which has been previously used for some other genus in the same kingdom.' It has been so generally understood that 'name' is to be taken in the philological sense of a district word, that no ruling appears to have been deemed necessary as to what really constitutes a name in a nomenclatorial sense; but usage—one may almost say universal usage—shows that words varying merely by endings denoting gender, or compound words differing only in the connective vowel, or in which certain consonants, notably *l* and *r*, are used single or double, or, in certain words of Greek origin, the retention or elimination of the aspirate, or the use of *i* in place of *y*, or *vice versa*, etc., do not constitute distinct words or 'names' in a nomenclatorial sense. In other words, it is held that names of genera must be etymologically distinct, however similar they may be in form or pronunciation. This is affirmed by the uniform practise of systematists for a century.

In view of the discovery in recent years of

the double employ of such a multitude of names in zoology, and the consequent wholesale elimination of those preoccupied though often of long currency; and also in view of the wide acceptance of the A. O. U. rule that names, generic or specific, 'are not to be rejected because of barbarous origin, for faulty construction, for inapplicability of meaning, or for erroneous signification,' and can be changed only to correct typographical errors, there has arisen a tendency to extend the rule of priority to the form of words, and to adopt names that vary to the extent of a single letter as tenable, whether etymologically the same or not. The first outbreak of this tendency, however, in code form, is furnished by the new ichthyological code, of which Canon XI., as given in *The Osprey,* reads:

"As a name is a word without necessary meaning, and as names are identified by their orthography, a generic name (typographical errors corrected) is distinct from all others not spelled in exactly the same way. Questions of etymology are not pertinent in case of adoption or rejection of names deemed preoccupied." The explanatory note following states that this canon "permits the use of generic names of like origin but of different genders or termination to remain tenable. All manner of confusion has been brought into nomenclature by the change of names because others nearly the same are in use. Thus the Ornithologists' Union sanction the cancellation of *Eremophila* because of the earlier genus *Eremophilus,* of *Parula* because of the earlier *Parulus,* and of *Helminthophaga* on account of *Helminthophagus.* On the other hand, *Pica* and *Picus* are allowed.* In ornithology this matter has been handled by a general agreement on the relatively few cases concerned. But in other groups, the matter is by no means simple, and every degree of similarity can be found."

* In this exceptional case of *Pica* and *Picus,* so often cited as an inconsistency, these two words are not gender forms of one name, but etymologically distinct words, used by the ancient classical writers as the names of two widely different birds, just as they are still used in ornithological nomenclature. Furthermore, it is a unique case.

This is the 'one-letter rule' *par excellence,* of which there have been mutterings of late in various quarters. Its promoters have good intentions, and high hopes, no doubt, that it will prove a panacea for an admitted evil. Possibly a beneficial compromise may result. When we reflect, however, that two forms of the same name, differing only by a single letter, sometimes occur in the same class, and often in the same branch, and that the same name when used for the same genus is current in several forms, differing sometimes more radically than by a single letter, and that, in many cases, the author of a name has himself used it at different times in all three genders, and sometimes in more than one gender in the same paper, and that many authors have in the past, and some still continue to exercise their own judgment or preference as to the correct gender of names, it seems hopeless to expect such a radical innovation to meet with general acceptance. By a slip of the pen or other lapsus even authors the most careful in such matters are sometimes caught using one form when they intended to use another. Many generic names have four to six variants that have been used for the same genus, while some of them may also have been current for wholly different genera. This seemingly should be enough to lay the goblin of the 'one-letter rule,' but it evidently is not, even with otherwise level-headed naturalists.

It would take too much space to illustrate the confusion and inconvenience that would arise from its serious adoption. For the full-fledged systematist illustration by concrete examples would seem to be superfluous.

It is a grievous inconvenience to have to abandon a long-current bird name or fish name for which one has almost formed an attachment as a household word, because some one has discovered that it had a prior use, perhaps only in a closely similar form, for some other genus of animals, perhaps insects, or mollusks, or cœlenterates, which had never before come within his horizon. In early days it was held that the same generic name could not be used for both animals and plants. The codes later ruled that there was no necessary connection between botanical nomenclature

and zoological nomenclature, and that the use of a generic name in the one kingdom did not debar its use in the other. The different branches of zoology have now become so extended and specialized that the same rule of divorce might well be extended to the different branches of zoology. Little, if any, confusion could arise to ornithologists, or mammalogists, or ichthyologists, if a bird name, a mammal name, or a fish name should have currency for a genus of insects, or mollusks, or crustaceans, or echinoderms, or in each of these branches. If it could be agreed—and I am aware of no opposition—that the same generic name may hold good in different branches of the animal kingdom, but must not be used twice in the same branch (as in vertebrates, for example), it would result in the restoration of not a few familiar names that have had to give way under the animal kingdom priority rule, and lessen, if not quite do away with the present incipient call for an impracticable 'one-letter rule.'

5. *The Authority for Names.*—It is difficult to see the reason for Canon XXIX., which appears not to be published in full in *The Condor.* It is contrary to current usage and to other modern codes, that the authority for a name, given in manuscript on a museum label, is to be cited as the proper authority for such names when published by another author, who supplies the description and assumes the responsibility for the species. This canon says: "If a writer ascribes one of his species to some one else, we must take his word for it. Thus the manuscript species of Kuhl and Van Hasselt in the Museum of Leyden, although printed by Cuvier and Valenciennes, should be ascribed to Kuhl and Van Hasselt." This is not only a confusion of responsibility, but is bibliographically misleading, tending to throw the investigator off the track in looking for the original description of the species. Unless the publishing author endorses the supposed new species, he simply ignores the manuscript name and takes the responsibility for its suppression, just as in the other case he takes the responsibility for its publication and supplies the necessary description. If the author of a manuscript name supplies a description to accompany it, which only rarely happens, and the publishing author uses it as inedited manuscript, then the author of the name is also the author of the description and is to be cited as the authority for the species. In the other case, the name should be cited, in synonyny, as Cuvier (ex Kuhl, MS.), and otherwise as simply Cuvier. In the case of inedited matter, the citation would be Kuhl (in Cuvier, etc.), and otherwise as Kuhl. This, like the other points criticized above, is a singularly retrograde step.

J. A. ALLEN.

CURRENT NOTES ON METEOROLOGY.

METEOROLOGICAL RESULTS OF THE BLUE HILL KITE WORK.

THE meteorological work done at the Blue Hill Observatory by means of kites has so often been alluded to in these 'Notes' that no comments on the value of this work are necessary at this time. The latest publication in this connection is a valuable report by H. H. Clayton, entitled 'The Diurnal and Annual Periods of Temperature, Humidity and Wind Velocity up to Four Kilometers in the Free Air, and the Average Vertical Gradients of these Elements at Blue Hill' (*Annals Astron. Obs. Harv. Coll.*, LVIII., Pt. I., 1904). Although some of the results herein discussed have already been brought forward in previous publications by Mr. Rotch and Mr. Clayton, the compact and careful summary now issued will be welcomed as giving a definite and complete presentation of the principal conclusions which have been reached through the well-known, extended and laborious series of scientific kite flights—a field of investigation in which Blue Hill has taken a front rank.

A study of the sources of error in the instruments and methods precedes the discussion of the results. Six possible sources of constant error are recognized as influencing the records, and also one source of error, not constant, which arises from temporary local differences of condition, and from the fact that the kites do not rise vertically. A glance at these preliminary pages will show with what extreme care the observations have been treat-

ed before being employed in obtaining any definite results. Mr. Clayton's thorough study of the sources of error must also bring up many doubts concerning the accuracy of results obtained by observers who exercise less care. It may be noted that, in Mr. Clayton's opinion, the excessive temperature gradients, greatly exceeding the adiabatic rate, which have several times been referred to in various publications, are probably due, for the most part, to the fact that the observations in question were not made simultaneously at the two levels (p. 14). Temporary local differences of temperature may also explain gradients which exceed the adiabatic rate (p. 15).

The interest which attaches to all reliable meteorological data obtained in the free air is so great as to warrant the inclusion, in the pages of SCIENCE, of the following summary of the most important points contained in Mr. Clayton's report.

Diurnal Period of Temperature at Different Heights.—On several occasions observations were obtained during many hours at heights of about 3 kms., but there was no evidence of any change of temperature due to a diurnal period. On June 18–19, 1900, for example, the temperature at a height of 2,900 ms. was recorded at intervals throughout twenty-four hours, and although there was a general fall under the influence of some general atmospheric change, there was no appreciable diurnal period (Fig. 3, p. 16), in spite of the fact that there were only a few cirrus clouds to obscure a small portion of the sky. At 1 km. there is a diurnal period of temperature, as is evidenced by numerous records, but with a tendency to a secondary maximum at night as well as by day. A marked feature is also a sudden fall of temperature after sunrise (about 9 A.M. in summer), the evidence from the movements of the kites at this time being to the effect that the diurnal convectional currents from the ground reach the kites then. This 'chilling' of the air at a height of about 1 km. is explained by Mr. Clayton as due to the rise of the ascending currents, on account of their inertia, to an altitude greater than their point of equilibrium. The ascending air is cooled by adiabatic expansion below the temperature of the air into which it penetrates; hence, at the tops of convectional currents of this kind, rising from the ground, there ought to be a belt of chilled air, above which there must be a higher temperature. Such an inverted temperature gradient is usually found above cumulus clouds. The diurnal change of temperature at the greatest altitude reached by the ascending currents must, therefore, be the opposite of that at the ground, *i. e.*, the temperature is lower by day than by night. The records of May 1, 1902, show clearly that an inversion of the march of the diurnal temperature does occur at the top of convectional currents rising from the warm ground (Fig. 4, p. 20), for while at 500 and 1,000 ms. the afternoon maximum is well marked, the temperature curve becomes inverted at 1,230 ms. At 2,000 ms. there is no perceptible diurnal period. This cooling at the tops of convectional currents begins nearer the earth's surface early in the morning, and reaches a maximum altitude about the warmest part of the day. The diurnal period of temperature at different heights is graphically summarized in Fig. 5 (p. 25), and verbally, on p. 29.

The Diurnal Period of Relative Humidity at Different Heights.—In general, the diurnal period in relative humidity is the inverse of that of the temperature at all levels up to and including 1,500 meters.

The Diurnal Period in Wind Velocity at Different Heights.—Mr. Clayton finds the well-known explanation, given by Espy and Köppen, of the diurnal variation in wind velocity only a partial one, for at night the air from 300 to 1,000 ms. above sea level does not merely resume a velocity of movement proportional to its height, but increases in velocity until its movement is more rapid than that of the air strata above or below the given level. Some other forces must, in Mr. Clayton's opinion, be called into play besides the retardation of the upper currents by ascending currents from below. It is suggested that, as the atmosphere is trying to maintain a mean velocity of flow having a constant value for the vertical section above any given point on the earth, if in any given part of

the section the velocity is diminished, the air must flow faster in some other portion. This theory seems to explain satisfactorily the increased velocity between 300 and 700 ms. at night. The retardation of the air between 200 and 700 meters during the day, due to ascending currents, results in an increased velocity near the ground, and, as this is not sufficient compensation, also in the section of air between 1,000 and 2,000 meters. Hence, at the latter height, the velocity has a maximum by day and a minimum by night, as is the case at the ground.

Vertical Gradients of Temperature, Humidity and Wind Velocity.—At night the temperature rises with increase of altitude up to about 500 meters, and not until a height of over 1,000 meters is reached is the temperature in the free air as low as at the ground. During the day the temperature decreases with altitude nearly at the adiabatic rate for dry air up to 500 meters. Above that height the rate decreases, probably owing to frequent inverted gradients and to cloud formation. Between 500 and 1,500 meters the temperature decreases more rapidly by night than by day. The decrease is most rapid in summer and least in winter. During the day the rate of decrease diminishes to 2,000 meters, and then increases again. From 0 to 500 meters the rate is at a maximum by day and a minimum by night, but between 1,000 and 1,500 meters this condition is reversed, owing to the inversion of the diurnal period. An important point, noted on page 50, concerns the *mean vertical temperature gradient*, about which much has been written. Gradients which are the mean of two opposing conditions may not occur at all. The most frequent gradients which actually occur are (I.) an increase of temperature with increase of altitude, between $+0^\circ.1$ and $+1^\circ.0$ (C.) per 100 meters, and (II.) the adiabatic gradient, $1^\circ.0$ (C.) per 100 meters. Some gradients exceeding the adiabatic rate have been observed, chiefly between 9 A.M. and 3. P.M. On the average, the relative humidity increases during the day up to about 1,000 meters, and then decreases to about 2,500 meters. During the night the relative humidity diminishes rapidly up to a height of 500 meters, and then more slowly, to a height of about 2,500 meters. Above 2,500 meters the relative humidity increases slowly again. There is a very rapid increase of wind velocity at night to a maximum at 500 meters, a slight decrease between 500 and 1,000 meters, and then an increase becoming more rapid with increasing height. There is a relatively rapid increase of wind velocity by day from the ground to 500 meters; a slower decrease from 500 to 1,500 meters, and almost no change from 1,500 to 2,000 meters.

R. DeC. Ward.

MEETING OF THE BRITISH ASSOCIATION IN SOUTH AFRICA.

The British Association will hold its meeting this year in South Africa. In these exceptional circumstances, the general officers of the association requested the council to appoint a strong committee to cooperate with them in carrying out the necessary arrangements. This 'South African Committee' has held frequent sittings; and its work is so far advanced that the London *Times* is now able to make the following announcements:

Although the annual circular and program have not yet been issued, pending the receipt of information from South Africa, many members have already intimated their intention of being present at the meeting. The 'official party' of guests invited by the central executive committee at Cape Town, and nominated in the first instance by the council of the association, numbers upwards of 150 persons, comprising members of the council, past and present general officers and sectional presidents, the present sectional officers, and a certain proportion of the leading members of each section. To this list has yet to be added, on the nomination of the organizing committee, the names of representative foreign and colonial men of science, the total number of the official party being restricted to two hundred, including the local officials. It is hoped, however, that many other members of the association will also attend the meeting.

The presidents-elect of the various sections are as follows:

A. *Mathematical and Physical Science.*—Professor A. R. Forsyth, M.A., Sc.D., F.R.S.

B. *Chemistry.*—T. Beilby.

C. *Geology.*—Professor H. A. Miers, M.A., D.Sc., F.R.S.

D. *Zoology.*—G. A. Boulenger, F.R.S.

E. *Geography.*—Admiral Sir W. J. L. Wharton, K.C.B., F.R.S.

F. *Economic Science and Statistics.*—Rev. W. Cunningham, D.D., D.Sc.

G. *Engineering.*—Colonel Sir Colin Scott-Moncrieff, G.C.S.I., K.C.M.G., R.E.

H. *Anthropology.*—A. C. Haddon, M.A., ScD., F.R.S.

I. *Physiology.*—Colonel D. Bruce, M.B., F.R.S.

K. *Botany.*—Harold Wager, F.R.S.

L. *Educational Science.*—Sir Richard C. Jebb, Litt.D., M.P.

The vice-president, recorders and secretaries of the eleven sections have also now been appointed.

In view of the numerous towns to be visited by the association, and in which lectures or addresses will be given, the number of lecturers appointed is much larger than usual. The list of these, as at present arranged, is as follows:

Cape Town.—Professor Poulton, on Burchell's work in South Africa; and Mr. C. V. Boys, on a subject in physics.

Durban.—Mr. F. Soddy, on radioactivity.

Maritzburg.—Professor Arnold, on compounds of steel.

Johannesburg.—Professor Ayrton, on distribution of power; Professor Porter, on mining; and Mr. G. W. Lamplugh, on the geology of the Victoria Falls.

Pretoria (or possibly Bulawayo).—Mr. Shipley, on a subject in zoology.

Bloemfontein.—Mr. Hinks, on a subject in astronomy.

Kimberley.—Sir William Crookes, on diamonds.

As the wish has been conveyed to the council from South Africa that a few competent investigators should be selected to deliver addresses dealing with local problems of which they possess special knowledge, a geologist, a bacteriologist and an archeologist have been invited to undertake this work, involving in two cases special missions in advance of the main party. Whilst Colonel Bruce, F.R.S., will deal with some bacteriological questions of practical importance to South Africa, Mr. G. W. Lamplugh (by the courtesy of the Board of Education) will be enabled to investigate certain features in the geology of the Victoria Falls, particularly as regards the origin and structure of the canon; and Mr. D. R. MacIver, who is at present exploring in Nubia, will proceed in March to Rhodes in order to examine and report on the ancient ruins at Zimbabwe and also Inyanga.

Most of the officials and other members of the association will leave Southampton on July 29 by the Union-Castle mail-steamer *Saxon*, and arrive at Cape Town on August 15, the opening day of the meeting; but a considerable number will start from Southampton on the previous Saturday, either by the ordinary mail-boat or by the intermediate steamer sailing on that date.

The sectional meetings will be held at Cape Town (three days) and Johannesburg (three days). Between the inaugural meeting at the former and the concluding meeting at the latter town opportunities will be offered to members to visit the Natal battlefields and other places of interest. Subsequently a party will be made up to proceed to the Victoria Falls, Zambesi; and, should a sufficient number of members register their names, a special steamer will be chartered for the voyage home, *viâ* Beira, by the East Coast route, as an alternative to the return through Cape Town by the West Coast route. Thus all the colonies and Rhodesia will be visited by the association. The tour will last seventy days *viâ* Cape Town, or a week longer *viâ* Beira (all sea), leaving Southampton on July 29, and returning thither on October 7 or 14.

A central executive committee has been constituted at Cape Town, with Sir David Gill as chairman and Dr. Gilchrist as secretary, while local committees have been formed at Johannesburg and other important centers.

Professor G. H. Darwin, F.R.S., is the president-elect; and among the vice-presidents-elect are the following: The Right Hon. Lord Milner, the Hon. Sir Walter Hely-Hutchinson, Sir Henry McCallum, the Hon. Sir Arthur Lawley, Sir H. J. Goold-Adams, Sir David Gill and Sir Charles Metcalfe.

Sir David Gill, Mr. Theodore Reunert and

others have taken a prominent part in the initial work. The South African Association for the Advancement of Science is cordially cooperating in the local organization, and will join with the British Association in attending the meeting.

The aim of the council has been to secure the attendance of a representative body of British men of science, including specialists in various lines of investigation; and that, along with the generous support of the people and authorities in South Africa, should go far to insure the success of the meeting and to stimulate local scientific interest and research.

JOINT ANNOUNCEMENT OF SUMMER FIELD COURSES IN GEOLOGY.

A PAMPHLET has lately been issued containing a brief account of the field courses in geology offered for the summer of 1905 by several universities in various parts of the United States. The number of courses offered and the professors, from whom information about them may be obtained, are as follows:

Intercollegiate Appalachian Course, Professor W. B. Clark.

University of Chicago, five courses, Professor R. D. Salisbury.

Columbia University, one course, Professor A. W. Grabau.

Harvard University, three courses, Professor J. B. Woodworth.

Johns Hopkins University, one course, Professor W. B. Clark.

University of Kansas, one course, Professor E. Haworth.

University of Minnesota, two courses, Professor C. W. Hall.

University of North Carolina, one course, Professor C. Cobb.

Ohio State University, one course, Professor C. S. Prosser.

Stanford University, two courses, Professor J. C. Branner.

University of Wisconsin, one course, Professor W. H. Hobbs.

In order to encourage the taking of summer field courses, the following colleges and universities have agreed to give credit, under certain conditions, to any of their students, who thus spent part of the vacation in scientific study:

Amherst College, University of Missouri, Beloit College, University of North Carolina, University of Chicago, Northwestern University, Colgate University, Oberlin College, Columbia University, Ohio Wesleyan University, Hamilton College, University of Rochester, Harvard University, Syracuse University, Johns Hopkins University, University of Toronto, University of Kansas, Vanderbilt University, Massachusetts Institute of Technology, Wesleyan University, McGill University, Western Reserve University, University of Michigan, Williams College, University of Wisconsin, University of Minnesota, Yale University.

SCIENTIFIC NOTES AND NEWS.

PROFESSOR SIMON NEWCOMB celebrated his seventieth birthday on March 12. Professor Newcomb is at present engaged in an important investigation, under the auspices of the Carnegie Institution, for determining the elements of the moon's motion and for testing the law of gravity.

PROFESSOR HENRI MOISSAN, of Paris, and Professor Wilhelm Ostwald, of Leipzig, have been elected corresponding members of the Berlin Academy of Sciences.

THE following candidates have been selected by the council of the Royal Society to be recommended for election into the society: John George Adami, William Arthur Bone, John Edward Campbell, William Henry Dines, Arthur Mostyn Field, R.N., Martin Onslow Forster, Edwin S. Goodrich, Frederick Gowland Hopkins, George William Lamplugh, Ernest William MacBride, Francis Wall Oliver, David Prain, George F. C. Searle, Robert John Strutt and Edmund Taylor Whittaker.

CAMBRIDGE UNIVERSITY will confer its doctorate of science on Dr. E. B. Taylor, F.R.S., professor of anthropology at Oxford.

ON the occasion of the opening of the new public health laboratory of the Victoria University, Manchester, honorary degrees were conferred upon Professor Calmette, Lille University; Professor Perroncito, Turin University; Professor Salomonsen, Copenhagen University, and Captain R. F. Scott, R.N.

PROFESSOR K. MÖBIUS has retired from the directorship of the Berlin Museum of Natural History. The position has been offered to

Professor H. H. Schauinsland, director of the museum at Bremen.

Dr. D. T. MacDougal, of the New York Botanical Garden, started on March 9 for Mellen, Arizona, and plans to make an examination of the deserts contiguous to that stream and the Gulf of California, and to obtain living material for the New York Botanical Garden. Mr. E. A. Goldman, of the Biological Survey of the Department of Agriculture, will accompany the expedition for the purpose of extending the field surveys of the department, and of obtaining material for the study of the fauna of the region.

Professor T. A. Jaggar, of Harvard University, will lead a geological expedition to Iceland during the summer. On or about May 25 the party will leave Boston for Liverpool. On June 10 it will leave Leith, Scotland, by steamer and will make a circuit of the island, stopping at places of interest on the coast, and finally landing at Reykjavik, whence a trip will be made northward over the island on foot or horseback. The party will return by steamer to Reykjavik and then to Leith after an absence of about forty days.

Mr. J. Maxwell Miller, Rhinehart scholar of the Peabody Institute, has modeled a bust in plaster of President Ira Remsen and presented it to the Johns Hopkins University.

A bust of Dr. William Osler, to be executed in marble by Mr. Hans Schuler, has been presented to the Johns Hopkins University. It is said that Dr. Osler will leave for England on about May 17.

Dr. Victor Hensen, professor of histology and embryology at Kiel, celebrated his seventieth birthday on February 10.

Mr. W. H. Maw has been elected president of the British Astronomical Association.

The Isaac Newton studentship of £250 for encouragement of study and research in astronomy has been conferred upon Mr. F. J. M. Stratton, B.A., scholar of Gonville and Caius College, Cambridge University.

The Prix Lacaze, of the value of 10,000 francs, awarded every four years by the Paris Faculté de Médecine to the author of the best work concerning tuberculosis, has been given to Dr. André Jousset.

Dr. Alexander Macfarlane will give at Lehigh University a course of lectures on British mathematicians of the nineteenth century as follows:

April 7, 11:30 A.M.—'Sir George Biddel Airy (1801–1892).'

April 8, 11:30 A.M.—'John Couch Adams (1819–1892).'

April 11, 5:00 P.M.—'Sir John Frederick William Herschel (1792–1871).'

April 13, 5:00 P.M.—'Isaac Todhunter (1820–1884).'

April 14, 11:30 A.M.—'Duncan Farquharson Gregory (1813–1844),' 'George Green (1793–1841).'

April 17, 5:00 P.M.—'George Salmon (1819–).' Conclusion.

The secretary of war, the Hon. Wm. H. Taft, has accepted the invitation of the National Geographic Society at Washington to address the society on the Philippines. The address will be given during the first week of May and is the last of ten addresses on the far east which the National Geographic Society arranged for 1905. The other addresses are: 'China,' by Hon. John W. Foster, ex-Secretary of State; 'Japan,' by Baron Kentaro Kaneko, of the House of Peers of Japan; 'Russia,' by Hon. Charles Emory Smith, formerly minister to Russia and ex-Postmaster General; 'Manchuria,' by Col. W. S. Schuyler, who has recently returned after eight months with the Russian armies in Manchuria; 'The Evolution of the Russian Government,' by Dr. Edwin A. Grosvenor, of Amherst College; 'Recent Observations on the Russo-Japanese War, in Japan and Manchuria,' by Dr. Louis Livingston Seaman; 'The Japanese Side of the War,' by William E. Curtis; 'The Panama Canal,' by Rear Admiral Colby M. Chester, U. S. N., superintendent of the U. S. Naval Observatory; 'The Commercial Prize of the Orient and its Relation to the Commerce of the United States,' by Hon. O. P. Austin, chief of the Bureau of Statistics. These addresses are published in the journal of the society, *The National Geographic Magazine.*

MR. MALCOLM MORRIS was expected to deliver the Harveian lecture before the Harveian Society of London on March 9, the subject being some modern therapeutic methods in dermatology, with exhibition of cases treated by the X and Finsen rays.

DR. DAVID MURRAY, professor of mathematics and astronomy at Rutgers College from 1863 to 1873 and subsequently adviser to the imperial minister of education at Japan and secretary of the board of regents of the University of the State of New York, died on March 2, aged seventy-five years.

DR. AUGUST BORNTRÄGER, associate professor of chemistry at Heidelberg, has died at the age of eighty-five years.

HARVARD UNIVERSITY and New York University again unite with the Bermuda Natural History Society in inviting zoologists and botanists to spend six weeks in the temporary biological station located, as during the past two years, at the Flatts, Bermuda. It is expected that the date of sailing from New York will be July 1. Those who desire to take advantage of the opportunities offered by the station should send applications as early as possible, and not later than May 1, either to Professor E. L. Mark, 109 Irving Street, Cambridge, Mass., or to Professor C. L. Bristol, New York University, University Heights, New York City.

THE *Albatross*, of the Bureau of Fisheries, has returned to California after four months of deep sea explorations of the South Pacific, under the direction of Mr. Alexander Agassiz.

THE forestry department of the University of Michigan, through the kindness of Dean C. Worcester, secretary of the interior, Philippine Islands, who was a graduate of the university in 1889, has received a collection of herbarium specimens of the forest flora of the islands, which will form study material and assist in preparing some of the students of the forestry department for the Philippine Service.

AN expedition from Indiana University, in charge of John A. Miller, professor of mechanics and astronomy, and W. A. Cogshall, assistant professor of astronomy, will go to Spain to observe the total solar eclipse that occurs on August 30. At some point in northeastern Spain, on a favorable site chosen by Professor A. F. Kuersteiner, of the department of romance languages, who is now in Spain, they will install their instruments. This temporary observatory will include a horizontal photographic telescope about seventy-five feet long, having an aperture of eight inches. Into this telescope the sun's rays will be reflected by a mirror moving at such a rate that it will reflect rays in a constant direction. This telescope, with one exception, will have greater photographic efficiency than any telescope that has hitherto been used to photograph the sun during a total solar eclipse, and is designed to secure photographs of the corona on a very large scale.

FIELD COLUMBIAN MUSEUM, Chicago, has arranged a course of nine lectures upon science and travel, on Saturday afternoons in March and April, at three o'clock, as follows:

March 4.—'The Explanation of Indian Ceremonies,' Dr. G. A. Dorsey, curator, department of anthropology, Field Columbian Museum.

March 11.—'Giant Reptiles of North America,' Mr. E. S. Riggs, assistant curator, division of paleontology, Field Columbian Museum.

March 18.—'Extinct Mammals of North America,' Mr. E. S. Riggs, assistant curator, division of paleontology, Field Columbian Museum.

March 25.—'Aims and Methods of Bird Study,' Dr. N. Dearborn, assistant curator, department of ornithology, Field Columbian Museum.

April 1.—'Hawaiian Cruise of the *Albatross*,' Professor C. C. Nutting, professor of zoology, University of Iowa.

April 8.—'The Fertilization of Flowers by Insects,' Dr. F. H. Snow, professor of systematic entomology, University of Kansas.

April 15.—'Geographic Factors Involved in the Rise of Chicago,' Dr. J. Paul Goode, assistant professor of geography, University of Chicago.

April 22.—'How Rivers and Lakes became Stocked with Fishes,' Dr. S. E. Meek, assistant curator, department of zoology, Field Columbian Museum.

April 29.—'The Basketry of California,' Dr. J. W. Hudson, assistant in the department of anthropology, Field Columbian Museum.

UNIVERSITY AND EDUCATIONAL NEWS.

By the death of Mrs. George L. Littlefield, widow of George L. Littlefield, of Pawtucket, R. I., Brown University becomes the recipient of the bulk of the Littlefield estate, estimated at $500,000. The will provides that the corporation shall apply the money as it sees fit, except that $100,000 shall be used for the establishment of the George L. Littlefield professorship of American history.

By the will of William F. Milton, of New York, his estate will go to Harvard University on the death of Mrs. Milton. The daily papers state that it is worth between one and two million dollars.

Columbia University has received $100,000 from Mr. Jacob H. Schiff to endow a chair of social work, and the new professorship has been filled by the appointment of Dr. Edward T. Devine, general secretary of the Charity Organization Society, director of the School of Philanthropy and editor of *Charities.* This endowment makes possible the close affiliation between the School of Philanthropy and Columbia University.

The contest of the will of Mrs. Josephine L. Newcomb, who left more than $2,000,000 for the endowment of a college for women in connection with Tulane University, New Orleans, has so far resulted favorably to the interests of the college.

The regents of the University of Nebraska have recently voted $50,000 for the erection of the first wing of a building to accommodate the department of geology and the State Museum. The condition of the department at present is so overcrowded and is so subject to loss by fire that the curator has boxed and removed fifty tons of material during the past school year. This has been lowered for safe keeping in an unused steam tunnel running under the campus. In the hope and full expectation that the legislature of Nebraska will act favorably upon this recommendation of the regents, the Honorable Charles H. Morrill, founder and patron of the Morrill geological expeditions of the University of Nebraska, has offered the department an additional thousand dollars annually with which to pursue geological investigations both within and beyond the limits of the state. This will make it possible for the first time in several years to again resume the annual Morrill geological expeditions which were so fruitful of results from 1891 to 1901.

Williams College will ultimately receive $12,500 by the will of Mrs. Harriet A. Jones, of Chicago.

Mr. E. Whitley has given $5,000 towards the endowment of a chair of pathology at Oxford.

The Cambridge University convocation has voted to retain compulsory Greek in the 'little go' or entrance examination, the vote being 1559 to 1052. It is understood that a majority of the resident teachers preferred to make Greek optional, but the vote of convocation is largely decided by the country clergy who have qualified for the M.A.

Dr. E. O. Lovett, professor of mathematics of Princeton University, has been elected professor of astronomy to succeed Dr. C. A. Young, who has become professor emeritus.

Mr. Harold L. Madison has been appointed instructor in zoology in Brown University.

Dr. C. S. Gager, assistant in the laboratories of the New York Botanical Garden, is acting as instructor in botany at Rutgers College for the last half of the collegiate year.

Mrs. Cornelius Stevenson, president, and several other members of the board of managers of the Free Museum of Science and Art of the University of Pennsylvania have resigned owing to friction connected with criticism of some of the discoveries of Professor Herman V. Hilprecht.

Dr. Charles G. Rockwood, Jr., professor of mathematics at Princeton University, has resigned.

Dr. Spottiswoode Cameron has been appointed professor of public health at Leeds.

Dr. A. R. Cushny, of the University of Michigan, has been appointed professor of pharmacology and materia medica in University College, London.

Professor L. V. Vernon-Harcourt has resigned the chair of civil engineering in University College, London.